
DEO ET REGI

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Confiderable Parts of the WORLD.

VOL. LIV. For the Year 1764.

LONDON:

Printed for L. DAVIS and C. REYMERS,
Printers to the ROYAL SOCIETY,
againft *Gray's-Inn Gate*, in *Holbourn*.

M.DCC.LXV.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume. And this information was thought the more necessary, not only as it has been the common opinion, that they were published by the authority, and under the direction, of the Society itself; but also, because several authors, both at home and abroad, have in their writings called them the *Transactions of the Royal Society*. Whereas in truth the Society, as a body, never did interest themselves any further in their publication, than by occasionally recommending the revival of them to some of their secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviseable, that a Committee of their Members should be appointed to reconsider the papers read before them, and select out of them such, as they should

ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance or ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of nature or art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons, through whoſe hands they receive them, are to be conſidered in no other light, than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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PHILO-

PHILOSOPHICAL TRANSACTIONS.

I. *An Account of a Mummy, inſpected at* London 1763. *In a Letter to* William Heberden, *M. D. F. R. S. from* John Hadley, *M. D. F. R. S.*

Dear Sir;

Read Jan. 12, 1764.

ON the 16th of December 1763, Dr. Wollaſton, Dr. Blanſhard, Dr. Hunter, Dr. Petit, the Rev. Mr. Egerton Leigh, and Mr. Hunter, met at my houſe; that we might together inſpect a Mummy, which I had received from the Muſeum of the Royal Society.

Our intention was; to examine the manner, in which this piece of antiquity had been put together; to compare it with the accounts given of theſe preparations by ancient authors; and to ſee, whether there were any traces left of the ſofter parts; and, if ſo, by what means they had been preſerved. A greater

number of authors have written on this ſubject, than I was aware of; ſo that, in all probability, we have not made any new diſcoveries. I encloſe to you the reſult of our enquiry; and the few obvious reflexions which occurred. If they appear to contain any thing worthy notice, you will pleaſe to communicate them to the Royal Society. I am,

Dear Sir,

Your moſt obedient and

Obliged humble Servant,

John Hadley.

Charter-Houſe,
Jan. 10th, 1764.

The

THE Mummy, which is the ſubject of the following pages, is the firſt article in Dr. Grew's Catalogue of the rarities of the Royal Society. He informs us, that it was a preſent from Henry Duke of Norfolk; and was an entire one, taken out of the Royal Pyramids. He then proceeds to deſcribe the manner, in which the ſeveral parts were wrapped up; but this he has not done exactly: as moſt of theſe very parts had evidently never been opened, till we examined them: and were then found in a very different ſtate from that in which they are repreſented by him.

This Mummy had been greatly injured, before it came into our hands; the head had been taken off from the body; and the wrappers, with which they had been united, having been deſtroyed, the cavity of the thorax was found open towards the neck: and part of the upper cruſt, with the clavicles, having been alſo broken away, the heads of the *oſſa humeri* preſented themſelves covered with a thin coat of pitch.

The feet alſo had been broken off from the legs; and were fixed, by wires, to the end of the wooden caſe in which the Mummy lay.

The outward painted covering, which reached from the upper part of the cheſt nearly to the bottom of the legs; had been removed and faſtened on again by a great number of ordinary nails, driven up to the head into the ſubſtance of the Mummy. This had moſt probably been done by thoſe, who had orders ſome years ſince to repair it; and by this, and by the manner in which they had faſtened on the feet, they ſeem to have done their work in a moſt clumſy manner.

This whole external covering of the fore part of the Mummy consisted of several folds of broad pieces of linnen cloth; made to adhere together by some viscous matter, which had not yet lost its property: and the whole had received an additional degree of strength and substance from the coat of paint laid on. The figures, which were not entirely defaced, were so much of the same kind with those which the writers on this subject have described, as to make any account of them here needless: and, indeed, they were all so much injured, as to render a particular description of them very difficult, if not impossible.

There were not the lest remains of hair or integuments, on any part of the head; some parts of the skull were quite bare; particularly about the temporal bones: which had the natural polish, and appeared in every respect like the bones of an ordinary skull. To other parts of the skull adhered several folds of pitched linnen; which together were near half an inch in thickness: on removing them, they were found to have been in actual contact with the bone; so that the integuments must have been taken away, before the wrappers were at first applied.

The under jaw was lost: and the superior maxillary, sphenoïdal and ethmoïdal bones were broken away; the *foramen occipitale* was stopped up with pitch, with which also the inner part of the skull was lined; this seemed to have been poured in at the *foramen*, and made to apply to the several parts of the inside of the skull, by turning the head in different directions; the wave of the melted pitch from such motion appearing very plain. The inside of the skull was in many places covered very thinly; and, in some few, which

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the fluid pitch had miſſed, it was quite bare. The pitch, which ſtopped up the *foramen occipitale*, had on it the impreſſion of one of the *vertebræ* of the neck; and externally about the *foramen* adhered a conſiderable quantity of pitch.

The outward painted covering being removed, nothing but linnen fillets were to be ſeen: which encloſed the whole Mummy.

Theſe fillets were of different breadths; the greater part about an inch and a half, thoſe about the feet much broader: they were torn longitudinally; thoſe few that had a ſelvage having it on one ſide only; the uppermoſt fillets were of a degree of fineneſs nearly equal to what is now ſold in the ſhops for 2*s.* 4*d.* *per* yard, under the name of long lawn; and were woven ſomething after the manner of Ruſſia-ſheeting: the fillets were of a brown colour, and in ſome meaſure rotten. Theſe outward fillets ſeemed to owe their colour to having been ſteeped in ſome gummy ſolution; as the inner ones were in pitch.

The fillets immediately under the painted covering lay in a tranſverſe direction; under theſe, which were many double, they lay oblique, diagonally from the ſhoulders to the *ilia*. Under theſe the fillets were broader, ſome nearly three inches; and lay longitudinally from the neck to the feet, and alſo from the ſhoulders down the ſides; on which there was a remarkable thickneſs of theſe longitudinal fillets: under theſe they were again tranſverſe, and under theſe again oblique.

The fillets in general externally did not adhere to each other; but, though pieces of a conſiderable length could be taken off intire, yet (from the great age)

ſo

ſo tender was the texture of the cloth, that it was impoſſible regularly to unroll them.

As the outward fillets were removed, thoſe that next preſented themſelves had been evidently ſteeped in pitch, and were in general coarſer, in folds, and more irregularly laid on; as they were more diſtant from the ſurface. The inner filleting of all was ſo impregnated with pitch, as to form with it one hard black brittle maſs; and had been burned nearly to a coal. On breaking this, it appeared in many places as if filled with a white efflorefſcence: like that obſervable on the outſide of *pyrites*, which have been expoſed to the air. This efflorefſcence however had nothing ſaline to the taſte; and did not diſſolve in water: but inſtantly diſappeared, on bringing it near enough to the fire to be ſlightly heated; and was ſoluble in ſpirit of wine.

In the cavity of the *abdomen* we found ſeveral ſmall pieces of bone, which had the appearance of dry oak, mixed with crumbled pitch; under this was found more ſolid pitch, which adhered to the ſpine.

After cutting away the maſs of cloth and pitch which covered the *thorax*; we found, the arms had been laid ſtrait down by the ſides of the cheſt, and the ulna and radius bent upwards, and laid with the hands acroſs upon the breaſt, the right hand being uppermoſt.

The bones of the fingers were loſt; but the metacarpal bones were found, broken off, and fallen into the thorax.

The filleting, which went round the upper part of the body, included the arms alſo; but they had evidently

dently been firſt wrapped ſeparately, then laid up in the poſition in which we found them, and the hollows which they formed filled up with pieces of pitched cloth.

In the cavity of the thorax there was alſo a conſiderable quantity of crumbled pitch and ſplinters of dry bone; and, as in the progreſs of this examination we continually found, that ſome of the bones did, as we laid them bare, ſeparate into ſuch ſplinters; it is very probable, that this appearance was owing to the Mummy's having been handled in a rough manner, and much ſhaken by the perſons who had driven it full of nails, when they were employed to repair the outſide of it.

On our firſt opening a way into the *thorax*, we imagined the ribs were deſtroyed; but, upon a more accurate examination, they were found entire; but ſo bedded in the pitch, and ſo black, and burned into the maſs, as to make it difficult to diſtinguiſh theſe very different ſubſtances from each other.

The bones of the ſpine and of the *pelvis* were in the ſame ſtate with the ribs; only rather more burned.

There was a conſiderable thickneſs of hard ſolid pitch lining the cavity of the *thorax*; this had been evidently liquified, and poured in; and retained that gloſſy appearance on its ſurface, which is obſervable on pitch that is ſuffered to cool without being diſturbed.

On breaking through this hard cruſt of pitch, to examine the *vertebræ* and the ribs, the pitch, which was under this cruſt and neareſt to the bones, was crumbly and ſoft; and, on being expoſed to the air, grew perfectly moiſt, in a very ſhort time.

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The lower extremities were wrapped ſeparately in fillets to nearly their natural ſize, and then bound together; the interſtices being rammed full of pitched rags.

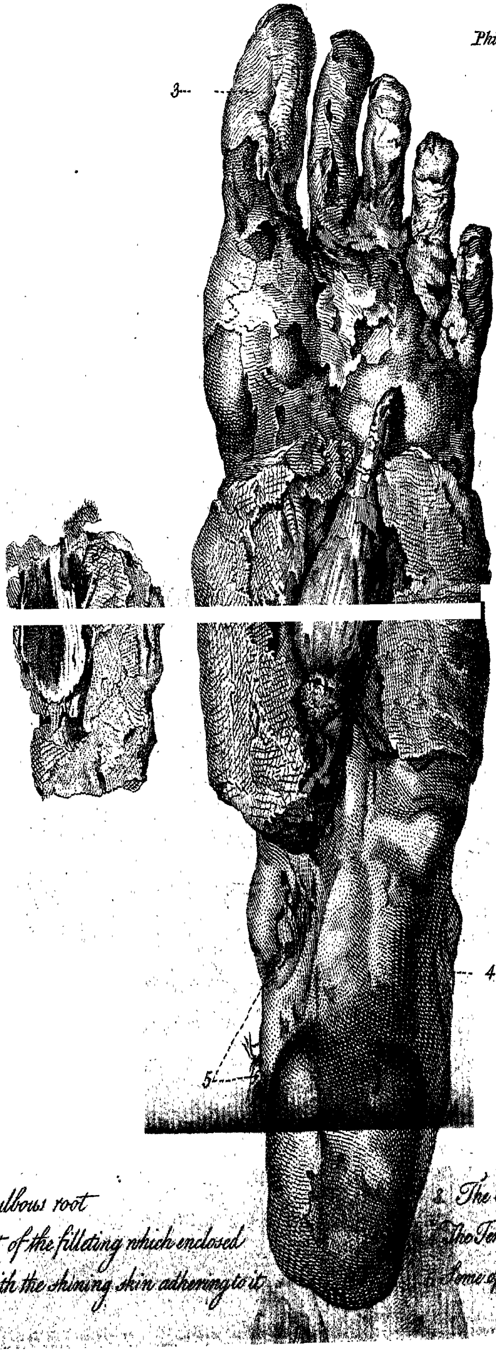
On cutting through the fillets on the thighs, the bones were found inveſted with a thin coat of pitch; and the filleting was bound immediately on this.

The *tibia* and *fibula* of each leg were found alſo wrapped in the ſame manner; and the bones in actual contact with the pitch: excepting in one or two places, where the pitch was ſo very thin, that the cloth appeared to adhere to the bone itſelf.

The feet were filleted in the ſame manner; being firſt bound ſeparately, and then wrapped together. The filleting had been by ſome accident rubbed off the toes of the right foot; and the nail of the great toe was found perfect: the laſt joints of the bones of the leſſer toes had been broken away; by which it appeared, that theſe bones had been penetrated and their cavities quite filled with pitch. The filleting about the heel had alſo been broken away, and the bones of the *tarſus*, and ſome of the metatarſal bones had fallen out and were loſt; leaving the remaining filleting like a kind of caſe.

The fillets on the left foot were perfect; except on the heel, and where they had been divided from thoſe of the leg; a ſmall portion of the *tendo Achillis* adhered to the *os calcis*; and ſome of the ligaments to the *aſtragalus*.

On cutting into the fillets on the ſole of this foot, they were found to encloſe a bulbous root. The appearance of this was very freſh; and part of the thin ſhining



1. The Bulbous root
2. The part of the filleting which enclosed the root with the shining skin adhering to it
3. The Spiral lines of the skin
4. The Tendons of ye Peroneus Anticus & Tibialis
5. Some of the Ligaments of the Tarsus

ſhining ſkin came off with a flake of the dry brittle filleting, with which it had been bound down; it ſeemed to have been in contact with the fleſh: the baſe of the root lay towards the heel. [Vide TAB. I.]

This diſcovery immediately brought to mind a paſſage in *Proſper Alpinus* *, and gave ſome appearance of probability to a relation, which, as he himſelf inſinuates, might give great reaſon to doubt his veracity. Speaking of the ſtone image of. a *ſcarabæus*, which was found in the breaſt of a Mummy he adds: " *Incredibile dictu, rami roriſma-* " *rini qui una cum idolo inventi fuerunt, folia uſque* " *adeo viridia et recentia viſa fuerunt, ut eâ die a* " *plantâ decerpti et poſiti apparuerint.*"

The fillets were removed from this foot with great care; they were much impregnated with pitch, excepting about the toes; where the ſeveral folds united into one maſs, being cut through, yielded to the knife like a very tough wax. The toes being carefully laid bare, the nails were found perfect upon them all; ſome of them retaining a reddiſh hue, as if they had been painted: the ſkin alſo, and even the fine ſpiral lines on it, were ſtill very viſible on the under part of the great toe, and of the three next adjoining toes. Where the ſkin of the toes was deſtroyed, there appeared a pitchy maſs, reſembling in form the fleſhy ſubſtance; though ſomewhat ſhrunk from its original bulk. The natural form of the fleſh was preſerved alſo on the under part of the foot; near the baſes of the toes. On the

* Proſper Alpinus rerum Ægyptiarum, &c; cum notis Veſlingii, 1735. pag. 36.

 back

back of the toes appeared ſeveral of the *extenſor* tendons.

The root juſt mentioned was bound to the foot by the filleting that inveſted the metatarſal bones; no more of this filleting was cut away, than was juſt ſufficient to ſhew, without removing from its place, a ſubſtance which had been preſerved in ſo extraordinary a manner.

On cutting away the fillets which covered the *tarſus*, the bones adhered ſtrongly together; and were covered with hard pitch: with which they ſeemed thoroughly impregnated.

On cutting away this outward pitch, there appeared very diſtinctly the tendons of the *peroneus anticus* and *poſticus*, the tendons of the *extenſor digitorum longus*, and the tendon of the *tibialis anticus*; and beſides theſe a conſiderable portion of the ligaments of the *tarſus*.

On examining the caſe formed by the pitch and fillets, which had covered the right foot, and out of which the bones had been taken; there was a very plain mould left, in which there had been encloſed another root ſimilar to that we diſcovered in the left foot; and in which ſome of the external ſhining ſkin of the root ſtill remained.

During this whole examination, if we except what was diſcovered in the feet, there were not found the leſt remains of any of the ſoft parts.

All the bones of the trunk were bedded in a maſs of pitch; and thoſe of the limbs were covered with a thin coat of it, and then ſwathed in the fillets: which (as has been mentioned) in ſome places where the pitch was very thin, ſeemed to adhere to the bone itſelf.

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The cavities of many of the bones, on being broken, were found quite full of this ſubſtance: the metacarpal bones were ſo; as were the *radii*, and many others: the ribs, as was before mentioned, were impregnated with it; and ſo burned, as to be with difficulty diſtinguiſhed from it: in which ſtate alſo, were the *vertebræ* and the bones of the *pelvis*.

The pitch had alſo penetrated into the cellular part of the head of the thigh bone; the ſmall bones of the toes were quite full: but it had not entered into all the metatarſal bones.

From experiment it has been found; that, bones and fleſh being boiled in common pitch, it will pervade the ſubſtance and fill the cavities of the former: and the latter will be ſo impregnated with it, as to be reduced to an uniform black brittle maſs; not in the leſt reſembling fleſh.

This treatment however will not account for the ſtate, in which this Mummy was found; for, if the fleſh had not been previouſly removed, though its appearance would have been entirely changed, yet the filleting could never have been found in contact with the bones.

From this laſt circumſtance it is moſt likely that the body, excepting the feet, had been reduced to a ſkeleton, before it was laid up; it is alſo pretty certain, that it muſt have been kept ſome time in boiling pitch; both before and after ſome of the layers of the innermoſt filleting were laid on.

The feet ſeem to have been ſwathed, at leſt in part, before they were committed to the hot pitch: and this ſeems to have pervaded the bandages, the fleſh and the bones.

It has been imagined, that the principal matter used by the Ægyptians for embalming was the *aſphaltus*; but what we found was certainly a vegetable production. The ſmell in burning was very unlike that of *aſphaltus*; nor did it reſemble that of the common pitch of the fir-tree: being rather aromatic.

It was compared with a variety of reſins and gum-reſins; but ſeemed not to reſemble any of them, excepting myrrh; and that but very ſlightly.

In all probability, it was not a ſimple ſubſtance; but might be a mixture of the reſinous productions of the country, with the pitch of that tree which they had in greateſt plenty.

The Αλειφαρ τȢ ΚεδρȢ of *Herodotus* *, and the Κεδρια of *Didorus Siculus* †, was moſt probably the tar of the cedar; it is the ſubſtance ſaid by theſe authors to be uſed for embalming; *Galen* ‡ mentions its power of preſerving bodies; and § *Dioſcorides* calls it ΝεκρȢ ζωη. *Pliny*, ſpeaking of the cedar, ſays; that the tar was forced out of it by fire, and that in *Syria* it was called cedrium: *cujus tanta vis eſt, ut in Ægypto* ‖ *corpora hominum defunctorum eo perfuſa ſerventur*.

Some branches of the cedar were procured from the phyſic garden at *Chelſea*; and, being treated in the manner deſcribed by *Pliny*, yielded tar and

* Herodot. Euterpe, pag. 119. ed. Gronov.

† Diodor. Sicul. lib. i. p.8 2. ed. Rhodomanni.

‡ Galen. de ſimpl. Med. Facult. lib. vii. cap. 16.

§ Dioſcorides de mat. medic. lib i. cap. 105. pag. 56. Francof. 1598.

‖ Plinii Hiſtor. lib xvi. cap. 11. pag. 382. ed. Dalecamp.

pitch

pitch, which had no aromatic ſmell, and ſeemed in many reſpects ſimilar to the produce of the fir-tree. There muſt undoubtedly therefore have been ſome other reſinous matter mixed with the *cedrium*.

The pitch of this Mummy was carefully diſtilled; but gave no other produce, than what might be expected from a reſinous body; the *caput mortuum*, when burned and elixated, yielded a fixed alkali; to this may be attributed the moiſture, which the pitch, that was in contact with the ſpine and thoſe other parts which were moſt burned, contracted on being broken and expoſed to the air; for this pitch had an alkaline taſte, and had been more than melted; having been burned to a *caput mortuum*.

A great variety of experiments were made on this pitchy matter; the reſult of them all tended to prove, that it had not the leſt reſemblance to *aſphaltus*; but was certainly a vegetable reſinous ſubſtance.

Monſ. *Rouelle*, in the Memoirs of the Royal Academy of Sciences for 1750, has given us a very elaborate and ingenious treatiſe on embalming: wherein he has chemically analyſed the pitch of ſix different Mummies.

From his obſervations; from what *Pietro della Valle**, and *Joannes Nardius* † at the end of his edition of *Lucretius*, have written on this head; from

* Viaggi di Pietro della Valle, Tom. 4.

† *Lucretius Joannis Nardii* de Funeribus Ægyptiorum Animadverſio 50. p. 627. Theſe accounts of della Valle and Nardius are alſo to be met with in the 3d vol. of Athanaſ. Kircher's Oedipus Ægygpt.

what Dr. *Middleton* ‡ obſerved in the Mummy which was opened at *Cambridge*; from the Memoires of Count *Caylus*, in the 23d vol. of Acad. des Inſcript. et Belles Lettres; and from this preſent examination; it appears, that various methods of embalming were practiſed among the *Ægyptians*; and that they uſed different materials for this purpoſe: and though *Herodotus* and *Diodorus Siculus* have given us reaſon to expect to find the bodies in a much more perfect ſtate, than we ever do meet with them, yet, on the other hand, it is evident; from the foot of this Mummy which we examined, and from the account Monſ. *Rouelle* and Count *Caylus* have given us in the above mentioned Memoires; that all the fleſhy parts were not always previouſly deſtroyed.

‡ Middleton's works, vol. 4. Germana quædam Antiquitatis monumenta.

II. *The Sequel of the Caſe of Mr.* Butler, *of* Moſcow, *Printed in* Philoſophical Tranſactions, Vol. L. p. 19. *Communicated by Mr.* Henry Baker, *F. R. S.*

Read Jan. 19, 1764.

IN the year 1757 I had the honour to communicate to this Royal Society, from Dr. James Mounſey, F. R. S. late Privy-counceller and chief Phyſician to the Empreſs of Ruſſia, the very extraordinary caſe of Mr. Butler, a paper-ſtainer at Moſcow, who, on mixing verdegris and falſe-gold leaf with aqua fortis, by way of experiment, for the production ~~of colours,~~ and ſtirring the mixture with a ſmall pair of ſciſſars, was ſoon after affected with a burning pain, firſt in one finger, then in his whole hand; afterwards in the other hand: in his legs, toes, ſhoulders, back, belly, forehead, eyes, and by turns in every part of his body, ſhooting from place to place, like flaſhes of lightning, as he termed it. He complained likewiſe of great anxiety and pain at the pit of the ſtomach, as if a burning iron was laid on it, with many other dangerous and uncommon ſymptoms, which in about ten days abated, and after a time ſeemed intirely gone: for the particulars whereof, and the manner in which he was treated by Dr. Mounſey, I muſt refer to the account printed in the 50th Vol. of the Philoſophical Tranſactions, p. 19.

Thus much being neceſſarily premiſed, I now take the liberty to lay before you a letter to me from Dr. Mounſey,

Mounſey, with a farther account of the caſe of this ſame Mr. Butler.

Dr. Mounſey's Letter to Mr. Baker.

Dumfries, Mar. 4th, 1763.

Dear Sir,

I AM aſhamed that I have delayed ſo long performing my promiſe to ſend you the ſequel of Mr. Butler's caſe, which you thought would be very agreeable to the Royal Society: but I partly waited to ſee if any thing farther remarkable would follow, and alſo I was *for a long time* after that ſo hurried with affairs, that I ſcarce had a moment to myſelf. I remember you wrote me word, that the Society does not much attend to theory and conjecture; ſo I ſhall omit to give my opinion of cauſes and their way of operating, and only ſend a ſimple narration of facts, in the ſame order they happened, extracted from my notes taken upon this occaſion.

In my former account of Mr. Butler's caſe, it is ſaid, that he had recovered his perfect health and ſtrength: yet after that he was often ſubject to ailments of the nervous kind, and became ſenſibly affected not only by the ſmell of paints, but even the handling of ſome kinds of metallic inodorous bodies gave him anxiety, tremor, faintings, and many other uneaſy ſymptoms.

The handling of verdegris, vitriol, and the like, threw him into theſe diſorders; and he aſſured me, that the handling copper or iron had the ſame effect

on

on him. — I often heard his complaints; but, as I deemed them imaginary, or sensations raised by the apprehension, I oftentimes only strove to undeceive his fancy.——However, I began to see, by some accidents, that there was more reality than I had believed, and that his first accident had left a disposition of the body susceptible to such impressions.

One day having got home a box of Cerussa, he took out some lumps to examine the quality, and handled them without the least suspicion of harm; but in a few hours after this he was taken with anxiety, palpitation of the heart, and a sense of trembling and weakness of the whole body. He was obliged to go to bed: he took some spirit of Hartshorn, sweated most plentifully, and next day was recovered. Many things of this sort happened to him: but I shall only give you an account of the most extraordinary attack which happened to him June 26th, 1758.

Mr. Butler still wanting to make experiments, but not daring to meddle with the operations himself, directed his wife to make some compositions of blue vitriol, alum, quick-lime, burnt alabaster, and things of this kind. — They were boiled in six several pots, then let stand some time, and the thin or watry part poured off. She brought these pots to her husband to look at: He was fond to try the colours himself, and, without any apprehension, he took some of those precipitations out of each pot, with the middle finger of his right hand, and rubbed them on grey paper to try the colours. He then put them away, and thought nothing more of the matter, drank tea, and was very well till about three hours thereafter. Then he began to be uneasy, and found pain in his

 arms,

arms, and eſpecially in his right hand; he became ſick at ſtomach, and felt a trembling over his whole body. He ſtrove to get the better of this attack, and, walked ſlowly about for ſome time, but turned pale faint, and fell down. He ſoon recovered again, and, ſtill thinking to maſter the illneſs, drank two or three glaſſes of wine, which he vomited up again. This began at noon, and at ſix in the evening I found him in bed frighted and ſweating. His pulſe was then regular but quick: he was ſick at ſtomach, with anxiety. I ordered him ſome ſaline draughts, and plenty of thin warm liquors. In the night he ſleeped but indifferently: his complaints were not continual, but returned by fits, with ſtretchings of the limbs, tremor, and ſtarting of the tendons over the whole body, and when he began to ſlumber he was diſturbed with frightful dreams of fire.

27th, Early in the morning he obſerved many ſmall purple ſpots on his hands. I found them juſt like purple petechiæ: the moſt on his right arm, and perceptible through the thick ſkin of the palm of that hand. There were alſo ſome on the other arm and legs, and ſome of a deeper colour on the thighs, but very few on the reſt of the body. As his pulſe was now grown quicker, I ſuſpected this to be a petechial fever: but there being no fevers of that kind then in town, and beſides, as I could not reconcile the other circumſtances, I remained undetermined and much perplexed by all theſe appearances. About four in the afternoon he was again ſeized with great anxiety, and pricking burning pains in the feet: the toes were extraordinary red, and he had frequent ſtretchings. Theſe went almoſt off in a few hours.

28th,

28th, He was not ſo much troubled with the frequent returns of his complaints: his pulſe was quick, and the ſpots kept out with itching.

29th, He was much the ſame as yeſterday, only more cheerful in the intervals, and the ſpots appeared fewer. He got frequently out of bed and walked in the room.

30th, The attacks returned much ſeldomer, and he would not keep in bed, but walked a great deal about the room, though his pulſe was ſtill feveriſh. Many of the ſpots diſappeared: moſt were grown pale, and ſome of a dun hue: Thoſe on the palm of the right hand were almoſt gone. — He ſaid, the ſpots grew always fairer every time the fits returned, and then he feltpricking pain with great heat, eſpecially on the inſides of his arms and legs, and in his feet and toes.

July 1ſt, I found him walking about the room, his pulſe ſtill quick. Laſt night he had been pretty eaſy and free. The ſpots were pale and diſappearing. He took a laxative, which operated very well.

2d, Laſt night he got pretty good reſt, but this morning the prickling and tremorous ſenſation over the whole body returned, but did not laſt long. He afterwards got up, walked about, and looked after his buſineſs. The ſpots were moſtly gone. He obſerved, that the pricking pains in his arms and legs, and in alarge ſpot on his back (which troubled him, in all his former accidents) came now only in the forenoons, and then almoſt ceaſed for the reſt of the day.

3d, Every thing much the ſame, but the attacks were lighter.

4th, Very little difference; only now and then he was troubled with a glowing painful ſenſation immediatly under the ſkin, ſometimes inone part of the body, ſometimes in another, a ſpot about the bigneſs of a crown piece.

5th, Things much the ſame.

6th, The attacks ſlighter, with the ſame feelings.

7th, Very little change, but rather better.

Hitherto I was moſtly an obſervator; and, not being forced by an abſolute neceſſity, I did not chuſe to load him, at an uncertainty, with many drugs. I had given him little more than abſorbent nitrous powders: but now, as he had no fever, but was rather lax and weak, and his nervous ſyſtem affected, I thought I might begin to give him things more powerful, and therefore ordered as follows:

℞. Extract. Cortic. Peruvian. drach. ij.
——— — — Aurantior.
Gum. Ammoniac.
—— Myrrh. ana drach. j
Sal. Martis drach. ſs.
Syrup. Cortic. Aurant. q. ſ. —F. Pillulæ ſingulæ gran. iv. deaur. ſig. capeat mane et veſperi v. Pillulas.

Here, in preſcribing, I had attention to the antipathy nature had ſhewn to iron, therefore took care the quantity in each doſe ſhould be very ſmall, the Sal martis ſcarcely making two grains.

8th, He took a doſe this night, was very reſtleſs, and greatly affected with all the former ſymptoms.

9th, He ſaid, he felt this medicine ſtruggling with the diſtemper within him; ſo ſwallowed, though with great

great reluctance, another dose early in the morning. In less than three hours he was again taken very ill, with anxiety, a sense of trembling over the whole body, and as if prickling sparks were flying out every where.—When I came to him, he begged me to change this medicine, and said it was like to have killed him. Having heard all his complaints, I made the pills be put away, and promised he should have no more of them: but his fear and aversion were so great, that the moment I was gone he ordered the box to be taken out of the house and thrown quite away.

10th, He passed this night tolerably, and found himself much better in the morning: but the complaints came by turns as before.

From this till the 20th, I gave him sundry medicines, but with little more effect than to ease him now and then: for the complaints always returned again in different manners and at uncertain times: but nothing extraordinary happened.

On the 20th, I gave him a dose of Epsom salt, which he had been used to take: it purged very well; but, immediately on its leaving off to work, his body struck out with great numbers of small red spots, without other inconvenience except a little extraordinary heat in the skin.

21st, The spots were almost gone, and he found himself more cool and easy than before.

22d, He took another dose, and the spots returned in the same way more than the first day: he found also the same relief. After this he took more doses of the same salt, always intermitting a day or two. The spots returned, but every time fewer appeared; and at last none appeared on taking these salts.

The

This ſal catharticum amarum came from England; and whether ſome vitriolic acid had been uſed in making it, I do not know; but it is likely there had.

From the firſt of Auguſt he took no medicines; the attacks were grown much leſs frequent and ſlighter: only he often felt in the night time like the ſtroke of an electrified body.

Auguſt 13. He was awaked this night by pain, as if burning irons had been clapped to the inſides of his legs, with anxiety and a ſenſe of tremor over the whole body. I was ſent for, but his complaints were greatly diminiſhed before I came. I found his pulſe very quick, irregular and ſmall: but I could find no new cauſe for the return of his complaints. He had after this ſome ſmaller attacks: but in the night of the 23d he was ſeized with a violent fit of the ſame ſort, with ſtretchings, and as if prickling ſparks were flying continually out of the ſkin. He had palpitation of the heart, and complained of the want of breath: his left ſide turned cold, and his right ſide grew hotter. When I came he was grown better, but the pulſe ſtill very ſmall, frequent and inordinate.

24th, He was again attacked in the ſame manner in the night time, and it alſo went off in the ſame manner: but he now grew feveriſh and kept his bed ſome days.

By the word *ſtretchings* I mean the ſtretching of his body and limbs by a ſlow and gentle convulſion of the extenſor muſcles; for in all the attacks I never obſerved the flexores any way affected. — His feelings were frequently ſo odd that, he ſaid, he could not deſcribe them. He often felt as if his left ſide, from

from his head to his wafte was empty, and that millions of fmall bodies were drove up and down with great velocity: which he likened to the fhaking of peas in a bladder.

I tryed many kinds of remedies to rid him of this diforder. He found often relief from them, but the ailments returned again. The remedy I found the moft effectual was my putting him on a milk diet, and making him drive hard on a cart every day, forenoon and afternoon, which he continued to do for feveral weeks. His complaints all decreafed; and, when he was threatened with an attack, a few drops of fpirits of hartfhorn and lavendar, or the like, were now of fervice to him, which formerly had no effect. In fhort, I gave him again animal food, and he kept his health pretty well.

The firft year after this he was always fearful; and often complaining, of what appeared to me fmall things, but by little and little he got the better of thefe alfo. Though he always continued to avoid handling metals, minerals, or things painted with thefe bodies. When I left Ruffia, he was very well; and I have lately heard by a letter that he continues fo; and I believe obferves the fame circumfpection about paints and metals as before.

III. *The Description of a new and safe Crane, which has four different Powers; invented by Mr.* James Ferguson, *F. R. S.*

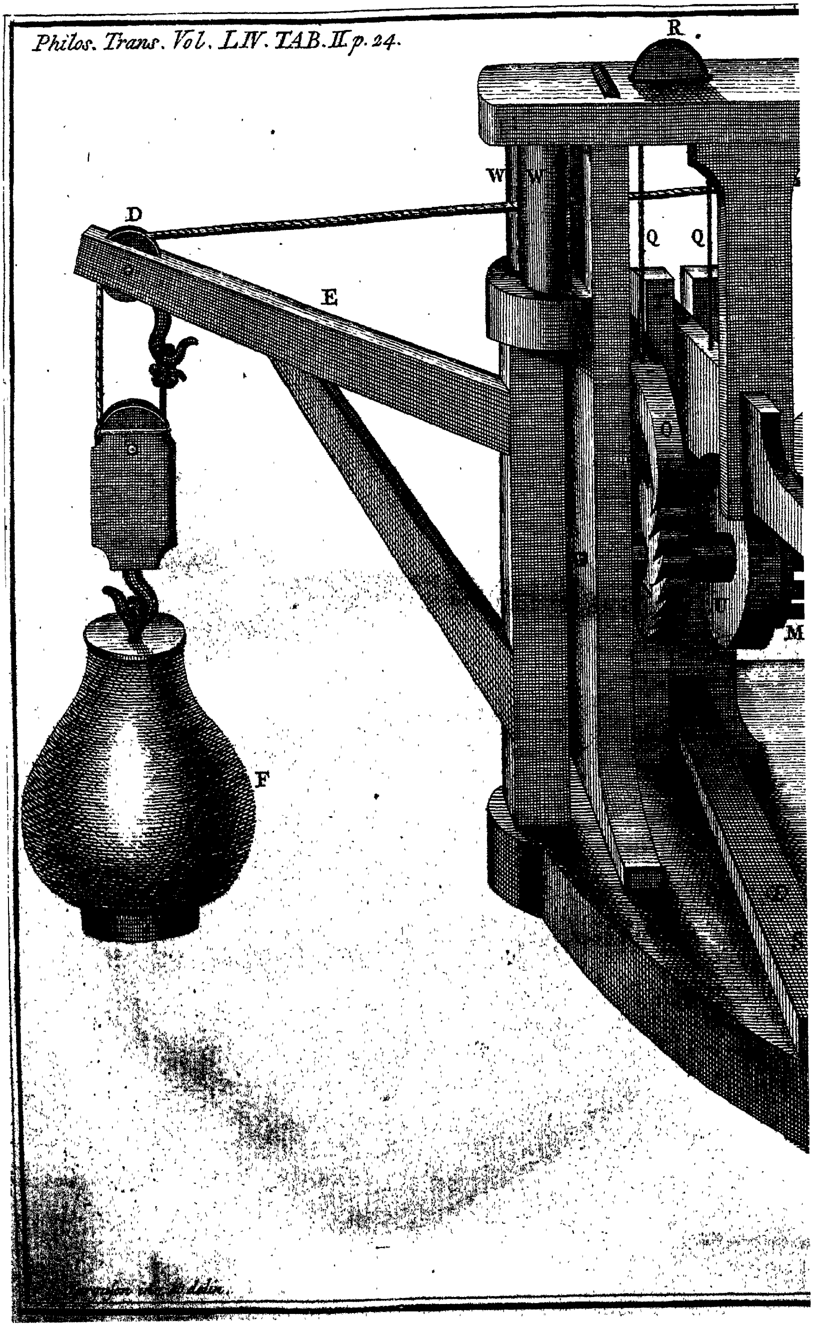
Read Jan. 19, 1764

THE common crane consists only of a large wheel and axle; and the rope, by which goods are drawn up from ships, winds or coils round the axle, as it is turned by men walking in the wheel. But, as these engines have nothing to stop the weight from running down, if any of the men happen to trip or fall in the wheel, the weight descends, and turns the wheel rapidly backward, and tosses the men violently about within it; which has produced melancholy instances, not only of limbs broke, but even of lives lost, by this ill-judged construction of cranes. And besides, they have but one power for all sorts of weights; so that, they generally spend as much time in raising a small weight as raising a great one.

These dangers and imperfections made me think of a method of remedying them. And for that purpose, I have contrived a crane with a proper stop to prevent the danger, and with different powers suited to different weights; so that there might be as little loss of time as possible: and also, that when heavy goods are let down into ships, the descent may be regular and deliberate.

This crane has four different powers: and, I believe, it might be built in a room eight feet in width; the gib being placed on the outside of the room.

Three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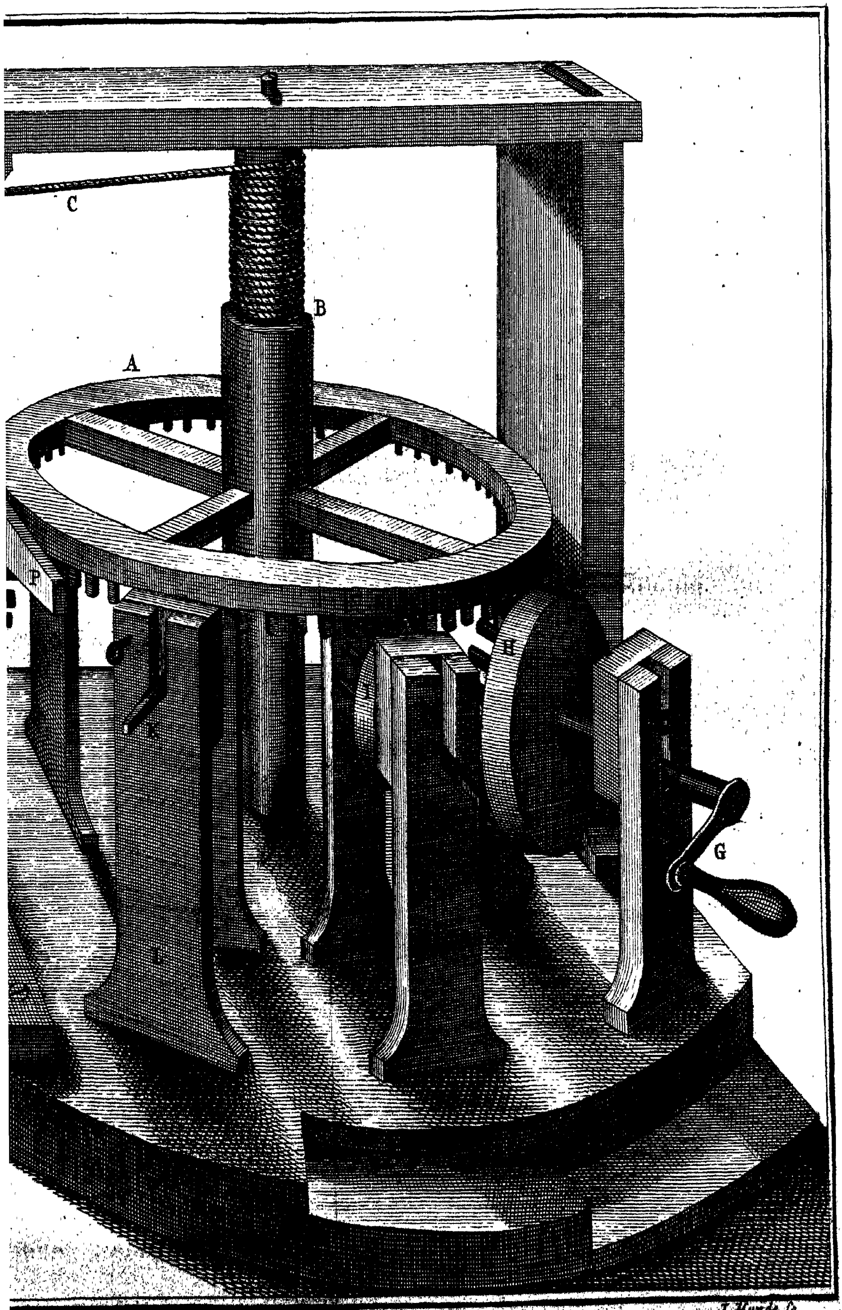


J. Mynde sc.

Three trundles, with different numbers of ftaves, are applied to the cogs of a horizontal wheel with an upright axle; and the rope, which draws up the goods, coils round the axle. The wheel has 96 cogs, the largeft trundle 24 ftaves, the next largeft 12, and the fmalleft has 6. So that the largeft trundle makes 4 revolutions for one revolution of the wheel and it's axle, the next largeft makes 8, and the fmalleft makes 16. A winch is occafionally put upon the axis of either of thefe trundles for turning it; the trundle being ufed that gives a power beft fuited to the weight: and the handle of the winch defcribes a circle, in every revolution, equal to twice the circumference of the axle of the wheel. So that the length of the winch doubles the power gained by the revolutions of each trundle.

As the advantage gained by any machine or engine whatever, is in direct proportion of the velocity of the power to the velocity of the weight; the powers of this crane are eafily eftimated; and are as follows.

If the largeft trundle be turned by the winch, it will make four revolutions for one revolution of the great axle on which the rope coils in drawing up the weight: and as the length of the winch is double the femidiameter of the axle, the power gained will be as eight to one: that is, a man will be able to raife eight times as much weight by means of the engine, as he could do by his natural ftrength without it; allowance being made for friction.

If the weight be too great for this power to raife, the fecond trundle may be turned by the winch, which will turn the wheel and axle twice as flow as

 the

the largeſt trundle did; becauſe it makes twice as many revolutions for one revolution of the wheel and it's axle: and then the power gained will be as ſixteen to one, becauſe the velocity of the power will be ſixteen times as great as the velocity of the weight.

If the weight be too great for this power to raiſe (which we ſtill ſuppoſe to be exerted by one man) the winch may be put upon the axis of the third (or ſmalleſt) trundle, and then, in turning the winch, the power gained will be as thirty-two to one.

But if the weight ſhould be too great, even for this power to raiſe, the power may be doubled by drawing up the weight by a double rope, going under a pulley in the moveable block which is hooked to the weight, below the arm of the gib; for then, the power will be as ſixty-four to one. If the block has two pullies, and the rope be twice doubled below them, the power will be as 128 to one: and ſo on, by adding more pullies, according to any required proportion.

Whilſt the weight is drawing up, the racch-teeth of a wheel ſlip round below a catch or click that falls ſucceſſively into them; and ſo hinders the crane from turning backward, and detains the weight in any part of it's aſcent, if the worker ſhould happen accidentally to quit his hold of the winch; or chooſe to reſt himſelf before the weight is quite drawn up. The catch, in this crane, is conſtructed much in the ſame way as in the great crane at Briſtol, invented by the late Mr. Padmore, of that city.

In order to let down a weight, the man who works the crane pulls down one end of a lever of the ſecond kind, which lifts the catch out of the ratchet-

wheel,

wheel, and gives the weight liberty to deſcend. But if the deſcent be too quick, he pulls the lever a little farther down, ſo as to make it rub againſt the round edge of a wheel, by which means he lets the weight go down as ſlowly as he pleaſes; and, by pulling a little harder, he can ſtop the weight, if needful, in any part of it's deſcent. If he accidentally quits his hold of the lever, the catch immediately falls, and ſtops the whole machine.

In the figure of this crane [TAB. I.] A is the great wheel, and B it's axle on which the rope C coils. This rope goes over a pulley D in the arm of the gib E, and is hooked to the weight F for drawing it up. G is the winch, H the largeſt trundle, I the next largeſt, and K is the axis of the ſmalleſt trundle, which is ſuppoſed to be hid from view by the upright ſupporter L. M is a trundle, which is turned by the great wheel; and on the axis of this trundle is fixt the ratchet wheel N, into the teeth of which the catch O falls. P is the lever, from which goes a rope QQ over a pulley R, to the catch; the end of the rope being fixed into the lever and catch. S is an elaſtic bar of wood, of which, one end is ſcrewed to the floor; and from the other end (out of ſight in the figure) goes a rope to the farther end of the lever, beyond the pin or axis on which it turns in the upright ſupporter T. The uſe of this bar is to keep up the lever from rubbing againſt the edge of the wheel U, and to let the catch keep in the teeth of the ratchet-wheel. But, when the end P of the lever is pulled down, it lifts the catch out of the ratchet wheel by means of the rope QQ, and gives the weight F liberty to deſcend:

 but

but if the lever be pulled a little farther down than what is ſufficient to lift the catch out of the teeth of the wheel, it will rub againſt the edge of the wheel V, and thereby hinder the too quick deſcent of the weight; and will quite ſtop the weight if pulled hard. And if the man ſhould happen inadvertently to let go the lever, the elaſtic bar will pull it ſuddenly up, and the catch will fall down into the wheel, and ſtop the machine.

W W are two upright rollers, above the axis or upper gudgeon of the gib E: their uſe is to let the rope bend upon them, as the gib is turned to either ſide, in order to bring the weight over the place to which it is intended to be let down.

N.B. The rollers ought to be ſo placed, that if the great rope were ſtretched *cloſe* by their outermoſt ſides, the *half* thickneſs of the rope may be perpendicularly over the center of the upper gudgeon of the gib. For then, and in no other poſition of the rollers, the length of the rope between the pulley in the gib and the axle of the great wheel will be always the ſame, in all poſitions of the gib; and the gib will remain in any poſition to which it is turned.

When either of the trundles is not uſed in working the crane, it may be drawn off from the wheel, after the pin near the axis of the trundle is drawn out, and the thick piece of wood is raiſed up a little, behind the outward ſupporter of the axis of the trundle. But this is not material: for, as the trundle has no friction on its axis but what is occaſioned by its own weight, it will be turned by the wheel without any ſenſible reſiſtance in working the crane.

IV. *Of the* Moon's *Diſtance and Parallax*: *A Letter to* Andrew Reid, *Eſq*; *from* P. Murdoch, *D. D. and F. R. S.* 12 Nov. 1763.

Read Jan. 26, 1764.

S I R,

I Have at your deſire wrote out what I was mentioning to you in our laſt converſation; of an eaſy rule for determining the Moon's diſtance, from the received theory of central forces: which I wiſh may merit your approbation: it will at leaſt ſerve as a teſtimony of the eſteem and regard with which I am, &c.

I.

Sir Iſaac Newton inveſtigated the law of gravitation, in the duplicate ratio of the diſtance of the central body inverſely, from the following *data*.

1. The length of a ſimple pendulum which vibrates in one *ſecond* of time, gave him, by Huygens' theorem, a determinate meaſure of the force of gravity, at the place of obſervation. And, by his own theory, he could thence infer the like meaſure for any other place, of a given latitude *.

2. The Earth's ſemidiameter was computed from the Abbé Picard's meaſure of a degree of the terreſtrial meridian.

* See one of the Eſſays prefixed to *Buſching's* Geography.

3. The

3. The Moon's parallax, as determined by the most skilful astronomers, gave him the Moon's distance in semidiameters of the Earth.

4. The time of a periodical month gave him the ratio of the versed sine of the arc of the Moon's orbit which she describes in one *second*, to the radius.

And from these his conclusion was; that the gravitation at the Earth's surface, being diminished as the square of the distance from the Earth's centre increases, would, at the distance of the Moon, produce a fall from rest, in one *second*, precisely equal to that versed sine. Or, that the gravitation of the Moon toward the Earth, being increased as the square of that distance is diminished, would, at the Earth's surface, be of the same quantity as that of falling bodies is (by the experiment of the pendulum) actually found to be.

II.

But the law of gravitation, thus deduced, being found to hold universally, and reciprocally, amongst all the great bodies of our system, so that even the minute anomalies of their motions are explained from it; we may now assume *it* as given, and make the *Moon's distance* the *quantity sought*.

Thus, writing F for the number of feet which a body falling from rest, describes, *in vacuo*, at the equator, in one *second*, V for the versed sine of the arc of the moon's orbit described in the same time, to the radius unity, D for the semidiameter of the equator in feet, and the ratio of the distance of the centers of the earth and Moon, to the semidiameter

of

of the earth, that of X to 1: We ſhall have, by the general law, the Moon's fall in 1″, equal to $\frac{F}{X^2}$; but the ſame fall is equal to $V \times D \times X$; whence $X^3 = \frac{F}{V \times D}$, and $X = \sqrt[3]{\frac{F}{V \times D}}$ is the diſtance ſought, in ſemidiameters of the equator.

Now a ſimple pendulum which beats ſeconds, meaſuring, at London, 39.126 inches; if the uſual allowance is made for the weight of the air, and for the *Newtonian* figure of the Earth *, the weight ($\frac{1}{289}$) taken off by the centrifugal force being likewiſe reſtored, a *ſecond-pendulum* at the equator would be 39.154 inches long. And, by Huygens' rule, half this length is to the initial fall in one *ſecond*, in the duplicate ratio of the diameter of a circle to it's circumference: that fall therefore, at the equator, and *in vacuo*, is 16.10185 feet; the logarithm of which number is 1.2068645 = log. F.

The *toiſes* in a degree of the equator, or, which is the ſame, in a degree of the meridian at *lat.* $54\frac{3}{4}$, being nearly 57200, the logarithm of the number of feet Engliſh in the ſemidiameter of the equator,

that is log. D will be nearly — —	7.3211900,
And the log. verſed ſine of the Moon's arc in 1″, being — — —	—12.5492882,
Their Sum — — — — —	—5.8704782

taken from log. F, leaves + 5.3363863, a third of which is 1.7787954, the logarithm of X = 60.08906 ſemidiameters of the equator.

* See the Eſſay quoted above.

And the arithmetical complement of this laſt logarithm, which is —2.2212046, is the log. tangent of the Moon's mean horizontal parallax at the equator; which therefore, is 57′ 12″, 34.

III.

Such would be the diſtance of the Earth's and Moon's centers, were the Earth immoveable: but it is ſomewhat increaſed by their revolution round their common centre of gravity.

Writing $x+1$ for that diſtance, divided by the centre of gravity in the ratio of x to 1; imagine a ſphere of the ſame dimenſions as our earth, placed at that centre, to exert the ſame attractive force on the Moon as our Earth actually does, the periodic time remaining unaltered: then muſt the denſity of this ſphere be diminiſhed in the ratio of x^2 to $\overline{x+1}|^2$, that its nearer diſtance from the Moon may be compenſated by the defect of denſity and attractive force. If now an inhabitant of the fictitious earth were ſuppoſed to compute its diſtance from the Moon, in the manner juſt now ſhewn; the quantities V and D would be the ſame as in the former calculation; but his f would be to our F, as x^2 to $\overline{x+1}^2$, and thence, his x would be to our X as $x^{\frac{2}{3}}$ to $\overline{x+1}^{\frac{2}{3}}$, that is, $x = \overline{\frac{x}{x+1}}|^{\frac{2}{3}} \times X$.

This is the diſtance from the fictitious Earth, or from the common centre of gravity; but (T) the diſtance from our Earth, is $\frac{x+1}{x} \times \overline{\frac{x}{x+1}}|^{\frac{2}{3}} \times X$, greater;

as was ſuppoſed, in the ratio of $x+1$ to x; that is,

$$T=\overline{\frac{x}{x+1}}\Big|^{-1}\times\overline{\frac{x}{x+1}}\Big|^{-\frac{2}{3}}\times X=\overline{\frac{x}{x+1}}\Big|^{-\frac{1}{3}}\times X=\sqrt[3]{\frac{x+1}{x}}\times X.$$

Sir Iſaac Newton, from the *phænomena* of the tides, eſtimated the ratio of $x+1$ to x to be that of 40.788 to 39.788. In that caſe, the cubic root of $\frac{x+1}{x}$ will have for its logarithm 0.0035934; which added to 1.7787954, the logarithm of X computed for an immoveable earth, gives 1.7823888, the logarithm of 60.5883 ſemidiameters of the equator. And the Moon's horizontal parallax, for this diſtance, is 56′ 44″,07.

IV.

On the other hand, if we had obſervations of the Moon's parallax (and diſtance) which could be reckoned exact enough for the purpoſe, we might thence determine the ratio of x to 1, that is, the ratio of the quantities of matter in the Earth and Moon.

For having $\frac{T}{X}=\sqrt[3]{\frac{x+1}{x}}$, and $\frac{T^3}{X^3}=\frac{x+1}{x}$; likewiſe T being given from obſervation, and X computed as above; it is manifeſt that the ratio of $x+1$ to x, and, by diviſion, that of x to 1, or of the maſs of the earth to that of the Moon, is given.

For example, if it ſhould be concluded from good obſervations, that T, the Moon's mean diſtance, is $60\frac{1}{2}$ ſemidiameters of the equator; for the logarithm of this diſtance, which is 1.7817554, take the logarithm of X, or 1.7787954, thrice the remainder

 will

will be 0.00888, the logarithm of $\frac{x+1}{x} = 1.02066$; and the maſſes of the Earth and Moon would, on this ſuppoſition, be as 48.4027 to 1.

In all this, a ſmall variation from the law of attraction, ariſing from the ſpheroid-figure of the earth, is neglected as inconſiderable; which it will be found to be by whoever takes the trouble to compute its quantity and effects.

R E M A R K S.

1. If F and D were taken of their juſt quantities, the Moon's horizontal parallax for an immoveable Earth being, at the equator, $57' \; 12\frac{1}{3}''$, is a *limit* which the true mean parallax cannot exceed: and the correſpondent diſtance 60.08906 is a *limit* which the diſtance cannot fall ſhort of: both being computed upon the ſuppoſition that $x+1=x$, or that the matter of the Moon is as nothing in compariſon of the Earth. Nor can the parallax and diſtance be ſuppoſed to lye very near theſe limits, without leaving too little attractive force in the Moon to raiſe the tides.

2. If the Moon's mean apparent ſemidiameter is $15' \; 38\frac{1}{4}''$, and the diſtance of the centers 60.5883 ſemidiameters of the equator, according to Sir Iſaac's eſtimate of the maſſes; the ſemidiameter of the Moon will be 0.275601 parts of the ſemidiameter of the equator, or .2763 of a mean ſemidiameter of the Earth. And the magnitudes of the Moon and Earth being as the cubes of their ſemidiameters, if the inverſe ratio of their magnitudes is joined to the direct

direct ratio of their maſſes (1 to 39.788) the ſum will be the ratio of their denſities, that of 1.19142 to 1, a little leſs than 6 to 5.

3. Suppoſing ſtill the ſame ſemidiameter of the orbit as before, the force of gravity will be to the Earth's attractive force on the Moon as 3670.94 to 1, and to the Moon's force on the Earth as 40.788 times that number, or 149730.4, to 1.

Again, the force of the Moon upon that ſurface of the ocean to which ſhe is vertical, being to her force on the Earth's centre, as the ſquare of 60.5883 to that of 59.5883; and the difference of theſe ſquares being to the latter as 1 to 29.54623, this difference of the forces will ſupport the weight of one 4423968th part of the water at the vertex. And, becauſe the Earth's ſemidiameter is ſmall in compariſon to the Moon's diſtance, the like differences of force will decreaſe from the ſurface to the centre, nearly in an arithmetical progreſſion, as the weight of the water does; making the caſe analogous to the diminution of gravity by centrifugal force.

But it is likewiſe eaſily ſhewn, that half this quantity of lunar force exerts itſelf to depreſs the waters all around at the diſtance of 90 degrees from the vertex; $\frac{3}{2}$ therefore of the former fraction, that is one 2949312 part of the force of gravity, will be the total cauſe of the difference in heigth of the *flood* and *ebb*, in an open and boundleſs ocean.

Say therefore, if (in determining the figure of the earth) $\frac{1}{289}$ of gravity, ſuſpended by the centrifugal force, gave, for the difference of diameters $\frac{1}{231}$, what will one 2949312 part give? and the anſwer, in feet, will be 8.887.

4. In

4. In like manner, if we take $8''\frac{1}{4}$ for the Sun's parallax, and, thence, his diſtance from the Earth 23468,6 ſemidiameters of the equator, we ſhall find that his *whole* force to produce a difference of *Flood* and *Ebb*, is to his force at the Earth's centre, as 1 to $7823\frac{2}{3}$. But the Sun's diſtance being to the radius of the Moon's orbit as 387.34535 to unity, this laſt force will be to that of the Earth on the Moon, as 387,34535 to 178.7234 (by cor. 2. prop. princip. I.) And the Earth's force on the Moon is to gravity as 1 to the ſquare of 60 5883; whence, adding theſe ratios, the Sun's force to move the ſea will be to the force of gravity as the fraction whoſe logarithm is — 8.1778026 to 1; or gravity is to that force as 13249445 to unity. And therefore, by the ſame analogy as above, we find the difference of *Flow* and *Ebb*, from the Sun alone, to be 1.97824; one foot $11\frac{3}{4}$ inches.

The ſolar force therefore, in raiſing the tides, is to the lunar, as 1 to 4.4924, in a ratio ſomewhat leſs than that computed by Sir *Iſaac*. The ratio likewiſe of the ſum of the forces to their difference is but 7.869 to 5, inſtead of 9 to 5, which he aſſumes from comparing the *ſpring* and *neap*-tides at *Briſtol*. And it is indeed ſurprizing, how he could, from that *datum*, arrive at concluſions ſo near the truth, as his very probably are. He tells us he uſed the ratio of 9 to 5, only till a more certain could be procured, And therefore the foreign mathematicians, who have cenſured him on that head, and on ſome other articles of this doctrine, might have ſpared their reflexions; at leaſt till they could ſhew that their own deductions were more agreeable to nature and obſervation.

6. Unity

6. Unity reprefenting the force of gravity, d the Sun's diftance, the Earth's force on the Sun will be $\frac{1}{d^3}$, or the fraction whofe logarithm is — 9.259021.8. And the folar force on the Earth is (from the numbers in remark 4) to the force of gravity as 1 to 1673.1: whence the attractive forces (and maffes) of the Sun and Earth will be as 325172,3 to 1. Add to this the inverfe ratio of their magnitudes, collected from the Sun's mean apparent femidiameter 16′.6″, and the parallax 8″$\frac{4}{5}$; and the denfity of the Sun will be to that of the Earth, as 1 to 4.068.

All this upon the fuppofition that the maffes of the Earth and Moon are as 39.788 and 1. Hereafter, when the Moon's diftance fhall be more certainly known, that element may be corrected, and the operations repeated.

As to the Sun's parallax 8″$\frac{4}{5}$, it cannot be much affected by any future determination of the Moon's diftance. Nor is it here affumed of that quantity, at random; but from a theorem deduced from the eftablifhed principles. I am, however, too diffident of myfelf to communicate it at prefent: becaufe, altho' it agrees very well with Mr. *Short's* conclufion from the *Tranfit* of *Venus*, it differs confiderably from that which a very learned and juftly celebrated author hath lately publifhed.

Note, The periods, as affumed in this paper, are; a *fidereal year* of 365.2563923 days; the periodical month 27.32165835 days.

V. *An*

V. *An Attempt to Account for the Origin and the Formation of the Extraneous Fossil commonly called the* Belemnite. [*Vide* TAB. III. IV. V.] *By Mr.* Joshua Platt.

To the Right Honourable George Earl of Macclesfield President, and to the Council and Fellows, of the Royal Society, the following Attempt to account for the Origin and true Formation of the Extraneous Fossil commonly called the Belemnite, is humbly addressed, by

Their much obliged and obedient

humble Servant,

Joshua Platt.

The

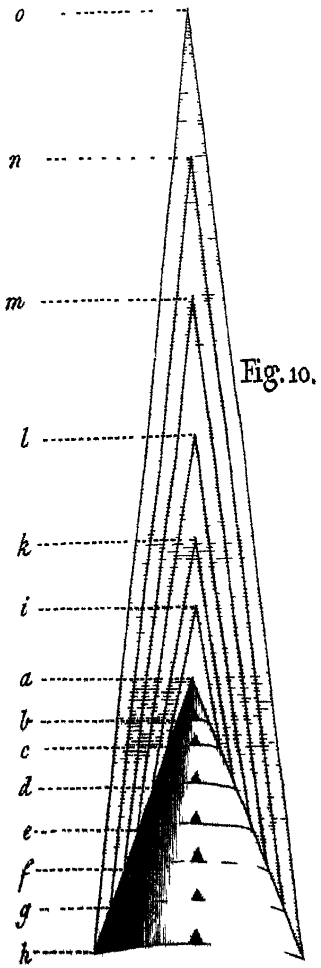


Fig. 10.

Fig. 6.

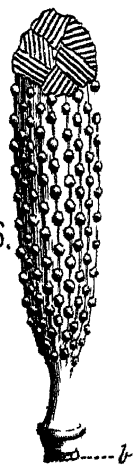
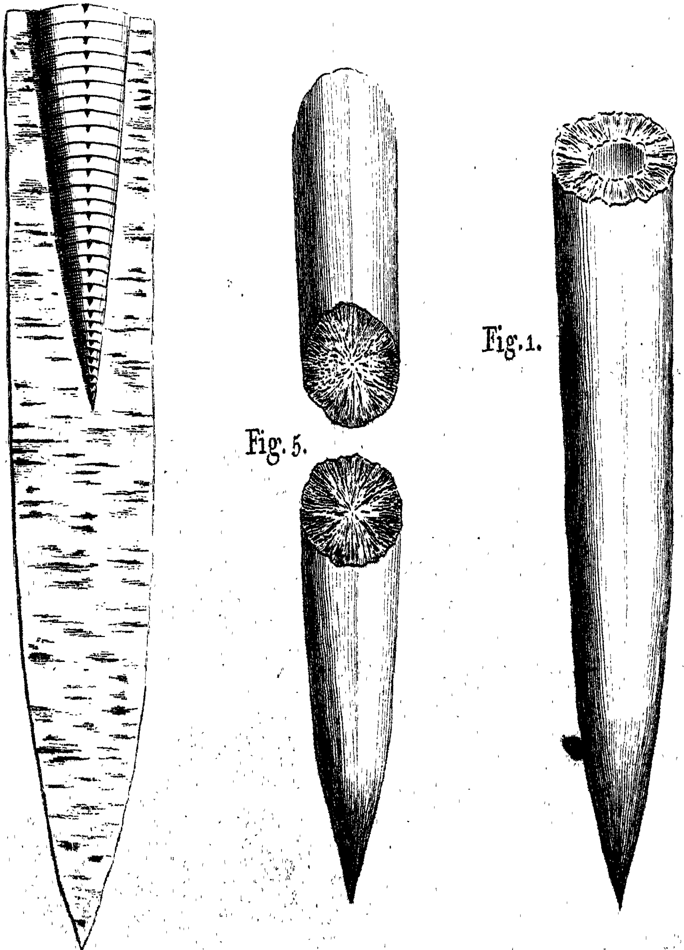


Fig. 11.



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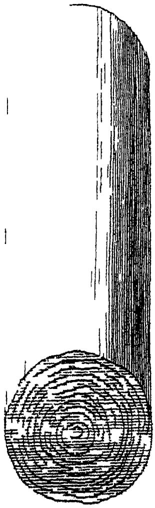
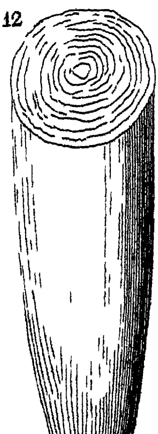


Fig. 13.

Fig 12



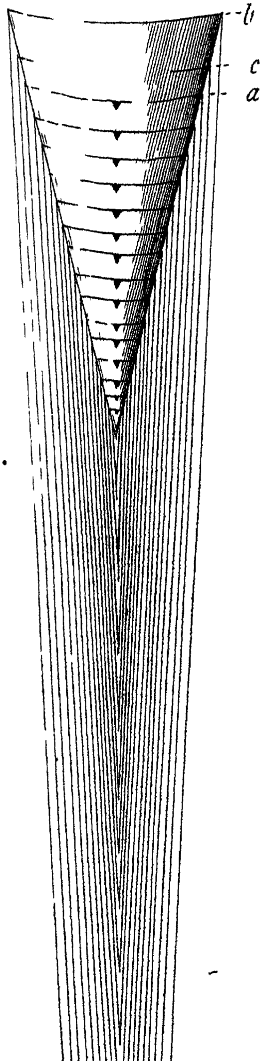


Fig. 14.

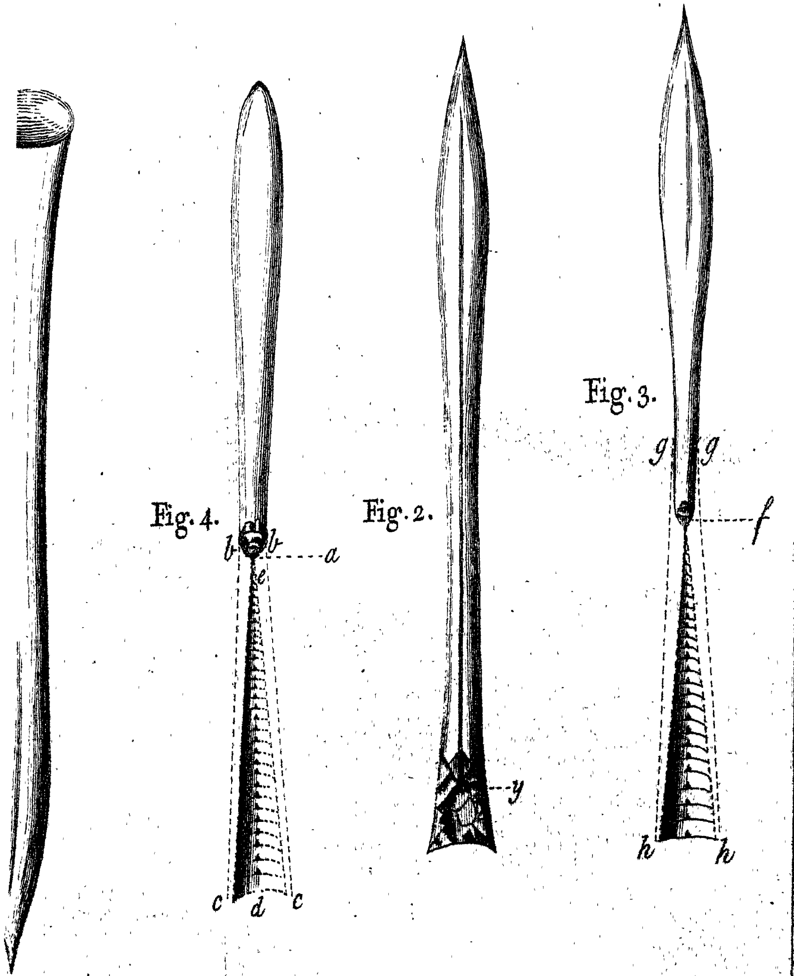


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Fig 17

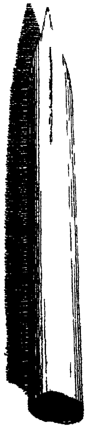
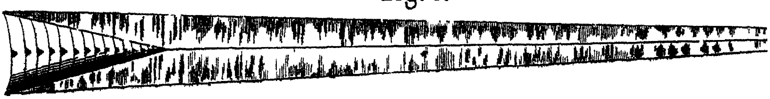


Fig. 15.

Fig. 8.



Found at Headington Stone pit near Oxon

Fig. 7.

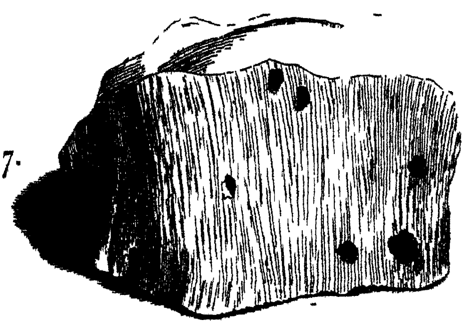


Fig. 16

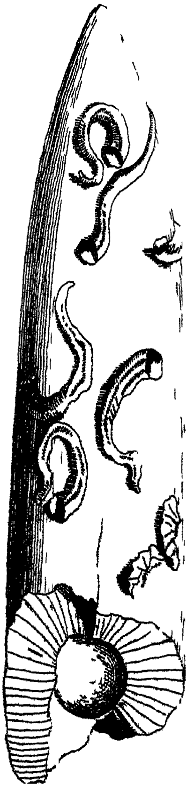
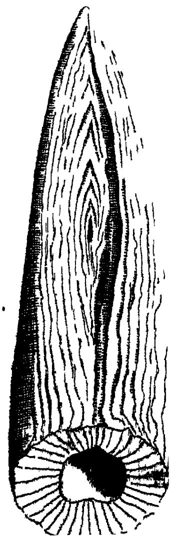


Fig. 9.



J Mynde sc.

Read Jan. 26, 1764.

THE public hath of late been agreeably entertained with descriptions of many curious Fossils discovered in different parts of this kingdom: but very little hath been offered with a view to ascertain their origin and formation; a point of much greater importance to a curious mind, than the most accurate descriptions, or the neatest delineations. It may indeed be thought unnecessary at this time, to say any thing of the origin of extraneous Fossils in general; all our modern naturalists being fully convinced, that they are the exuviæ or remains of animals and vegetables, and the greater part of them of marine production.

But as to their particular origin and formation; in what manner they were produced in the recent, and how and with what matter they afterwards became impregnated in their fossil state; all this is a field of natural inquiry, that has been very much neglected, notwithstanding it is the most fertile and productive of useful and entertaining knowledge. Besides, were we to consider it in this view, the recent and fossil remains would be found to throw a mutual light upon each other, and the naturalist would not be so often at a loss to class every new fossil acquisition, of which the recent specimen is not to be found, especially whenever the fossil has any thing seemingly æquivocal in its formation, so as on a superficial inspection to render the matter doubtful, whether the body belongs to the animal or vegetable kingdoms, or indeed to either of them. One of the first note is the Belemnite, which has not until very lately been even ranked amongst the marine productions;

but

but whoſe origin and formation have never yet been fully explained. I ſhall not enter into a minute detail of the ſeveral ſpecies of the Belemnite. The hiſtory of this extraneous foſſil, or an attempt to account for the origin and formation of the Belemnite, ſo far as they can be diſcovered and confirmed by reaſonings drawn from facts and experience, is the object of the preſent enquiry. I ſhall therefore confine myſelf to two ſpecies of the Belemnite; the one common in moſt counties of this kingdom, and vulgarly known by the name of Thunder-bolt [*a*]: the other that of the fuſiform or Spindlekind [*b*], found in ſlate-ſtone at Stons-field, but in far greater plenty in the clay near Piddington [*c*] Oxfordſhire; and in the chalk-pits of Kent and Surrey [*d*]. Thoſe in chalk have been often miſtaken for ſpines of the ſea-hedgehog, or Echinus Ovarius; but the characteriſtics of theſe two bodies are widely different. The Belemnite breaks in a direction perpendicular to its axis [*e*]: the ſpine obliquely [*f*]. The Belemnite, when broken, exhibits central rays; the Spine a ſmooth reſplendent ſurface. This diſtinction is invariable, if the trial be repeated a thouſand times. Theſe different appearances are probably the effects of different formations: and therefore the Belemnite ſeems to be formed by appoſition, and the Aculeus or Spine by protruſion, or, as Mr. Reaumur calls it, by intus-ſuſception. The radii in the Belemnite are owing to the fine laminæ, of which it is compoſed; they are ſo very thin, and break ſo nearly alike, that they have ever an horizontal ſurface when broken,

[*a*] Tab. III. Fig. 1. [*b*] Fig. 2. [*c*] Fig. 3. [*d*] Fig. 4. [*e*] Fig. 5. [*f*] Fig. 6.

which

which is common to all the ſhells of the trichite kind [*g*]. The ſpine being formed by protruſion, its component parts are adjuſted on a different manner, and the pores like the cancelli in bones (though not ſo diſtinct) are irregular, which is the reaſon of its breaking obliquely in any direction, but it is generally ſmooth by being ſaturated with a plated kind of ſpar [*h*].

My ingenious and very worthy friend Mr. Brander, in a diſſertation on the Belemnite, preſented to the Royal Society [*i*], juſtly obſerves; "that the Belemnite belongs to the teſtaceous part of the animal kingdom, and to the family of the nautili." And I would beg leave farther to add, that this gentleman's ſentiments are greatly ſtrengthened by the ſurpriſing analogy, which the Belemnite bears to the little pearly concamerated ſhell, or cornu Ammonis; and the orthoceratites, to the large nautilus; the former having its ſiphunculus upon the verge, as the latter has it in the center of the diaphragm, or partition, of each cell or chamber. "It has indeed been truly matter of ſpeculation (continues Mr. Brander) how this huge ſolid ſubſtance called the Belemnite, excluſive of the nucleus, could be formed; and how it happens, that ſome Belemnites ſhould have the nucleus within them, others not; the cavity to

[*g*] Fig. 7. A piece of the Penna Marina, perforated by the Pholades.

[*h*] Spar ſeems to be nothing but cryſtal debaſed by a calcareous earth: the more debaſed ſort breaks in a hairy trichite manner, the more pellucid kind with a ſmooth ſurface; and always in an oblique rhomboidal direction; which perhaps may in ſome meaſure enable us to account for its double refraction.

[*i*] Philoſophical Tranſactions, Vol. xlviii. for 1754, page 803.

contain the ſame in ſome very ſmall, in others ſcarce or not at all viſible".— But I flatter myſelf, that it will be found upon enquiry, that theſe are only circumſtances, which are common to other teſtaceous bodies, that have been accidentally broken or decayed by time, when forſaken by their inhabitants. For no teſtaceous body can be formed without an inhabitant; nor does it appear to me, that any Belemnite was ever formed without an alveolus, or concamerated ſhell.

The conical cavity and its nucleus are always proportioned to the bulk of the Belemnite, but not to its length: ſome are four times longer in proportion to the alveolus than others. The apex of the conical cavity, where the alveolus is firſt formed, in ſome runs up about half the length of the whole Belemnite; in others not a ſixth part of the whole [*k*]: but the aperture, or upper chamber [*l*] is equally proportionable to the bulk, or circumference of the Belemnite, of whatſover ſize or ſhape; and is the ſeat [*m*] or dwelling-place of the animal, that forms the Belemnite. In what manner this work is executed, I ſhall now endeavour to explain.

A conſiderable part of marine bodies, eſpecially thoſe of the teſtaceous tribe, are generally buried in mud or ſand, except ſome few, which ſtick to rocks, &c. as the limpets and periwincles; by which means we are prevented from making thoſe remarks upon the ſeveral ſtages of their growth, which an accurate enquirer would deſire. We muſt therefore have re-

[*k*] Fig. 8. [*l*] Fig. xiii. *b. c.*

[*m*] We never find a Belemnite with part of the alveolus, but the veſtigia or marks of the remainder appear in the cavity, and are continued to the verge of it.

courſe

courſe to the different ſteps or periods of their life and growth, as they are marked out by the indented lips or foldings of the ſhell; untill they arrive at their full ſize; when they begin to fortify themſelves, by bulwarks and ſtrong holds, againſt the injuries and incidents, which attend old age. This is moſt conſpicuous in the cowree, or concha Veneris of Liſter, book iv. ſect. 9.

Mr. Reaumur [*n*] found, by repeated experiments, that land ſnails form their ſhells by juxtapoſition: as the animal grows in bulk, the ſhell is increaſed by a mucous matter emitted from the body of the animal, which hardens by degrees into a teſtaceous ſubſtance: and from the experiments upon land ſhells that great naturaliſt concludes by analogy, that all teſtaceous bodies are formed in the like manner, particularly thoſe of the turbinated kind.

To this general rule an objection is made by Mr. Poupart, from the formation of the cowree, or concha Veneris before mentioned: but this learned gentleman was not aware, that this ſhell is firſt a buccinum, forming many convolutions before it draws in the verge to form the indented lip.

It was this very objection of Mr. Poupart, which led me to examine into the growth of the cowree; and by ſawing one of them through the middle, I found a turbinated ſhell within the outer wall, conſiſting of ſix or ſeven convolutions, but no ſtages, or periods, of the indented lip appeared in any of the convolutions, as we find in the helmet ſhell, and ſeveral of the buccinæ. I then began to conſider how this animal enlarged its dwelling; and was fully convinced, that

[*n*] See his book of inſects.

no more convolutions could be carried on; the indented lips being a full ſtop to its inward dimenſions; and that here was the period of its growth. My ſentiments were juſt as to its inward dimenſion; but obſerving that the lips of ſome were much larger than others, and that the curved part of the outer lip appeared thicker, when ſawed open, than the other parts of the ſhell; I began to think, that the animal, inſtead of enlarging the inner dimenſions, was employed in thickening the outer wall, to guard againſt injuries and accidents, ſo common to the inhabitants of that turbulent element the ſea. I was the more confirmed in theſe ſentiments by ſeeing the beautiful ſpots, with which this animal decorates its houſe, covered by other ſpots of different colour and ſize, as new laminæ were added to ſtrengthen the laſt-formed convolution. It is really matter of admiration to ſee how theſe ſhells are adorned and variegated; the exquiſite poliſh, which covers the whole infinitely ſurpaſſing the ſkill even of the moſt accompliſhed human artiſt. Theſe new coverings or laminæ, which are carried from the lips, terminate in the middle of the back part of the ſhell; and there form a liſt, or ſeam, of a quite different colour from that of the other part of the ſhell, and of an unequal ſurface.

This very circumſtance gave birth to my ſentiments concerning the formation of the Belemnite: for whoever conſiders the ſeam or ſulcus in the Belemnite, will, I think, conclude with me, that the outward lamina is formed lateſt, as in the cowree, and that the ſeam or ſulcus is cauſed by the ſeveral additional coverings or laminæ terminating there. But as the anatomiſt makes freſh diſcoveries by diſſecting the

ſubject,

ſubject, ſo (if I may be allowed the compariſon) I received farther information by luckily meeting with a Belemnite, whoſe laminæ were in a manner diſſected and laid open by the vague acid, or ſome other corroding menſtruum, which every where pervades the earth, deſtroying ſome bodies [o], and forming others [p]. The laminæ of this truly wonderful body are here expoſed to view [q], and plainly ſhew us, that nature, in this, as in all her works, purſues the moſt ſimple, eaſy, and ſhorteſt methods, though they appear ever ſo intricate and interwoven. This ſpecimen will, I hope, ſerve to explain a matter, which hath ſo long puzzled the curious in natural hiſtory; and convince us, that there is nothing more wonderful in the formation of the Belemnite, than in that of a cockle, oyſter, or any other teſtaceous ſubſtance; with this difference only, the oyſter ſtrengthens its ſhell, and excludes its firſt habitation, by additional laminæ formed *within*; the Belemnite incloſes its dwelling by adding new laminæ *without*. Figure X. repreſents the Belemnite ſplit up the middle, with the ſiphunculus in the front: *a*, *b* exhibit the firſt formed cell, or ſeat of the animal ab ovo. As the animal grows larger, it forms a ſecond cell or chamber *b* to *c*, at the ſame time covers the firſt cell, by forming the appendage or guard *c*, *i*, which is the firſt ſtage of the Belemnite. In forming the third cell *c*, *d*, freſh laminæ or coverings are carried on from *d*, to *k*; and ſo of the reſt, *e*, *f*, *g*, *h*; or *l*, *m*, *n*, *o*. When we have duly conſidered the manner, in which the ſhell is

[*o*] All calcareous ſubſtances.

[*p*] Such as ſelenites, pyrites, marcaſites, talk, gypſum, &c.

[*q*] Fig. 9.

thus

thus formed; it will be no difficult taſk to account for the different ſections and broken parts of the Belemnite; and in what manner they were reduced to the ſeveral forms or appearances, in which we commonly find them.

The better to illuſtrate my conjecture, I ſhall firſt exhibit ſome drawings, which ſhew the ſeveral ſpecimens broken and imperfect; and then propoſe my ſentiments concerning them before they were deſerted by their inhabitants. Figure 6. ſhews the ſpine of the echinus ovarius broken obliquely, as is common to all of them. Figure 5. exhibits the inward ſtructure of the Belemnite, when broken horizontally, with the central rays. Figure xi. is the ſame Belemnite ſplit through its axis. Figure xii, and xiii, are broken in the ſame direction as Fig. 5. and xi, and ſhew how the ſeveral laminæ [*r*] are placed one over another in the manner, in which it is formed. Fig. xiv. ſhews the Belemnite in the moſt perfect ſtate we ever find it. Fig. 4. is the fuſiform Belemnite found in chalk, which has been often taken for a ſpine. *a*, which is the termination of the conical cavity, has been thought to be the *ſocket* of the ſpine, which receives the papilla, when growing to the echinus; but, when compared with the ſocket of the true ſpine [*s*], we find it widely different. The pricked lines *b*, *c*. *b*, *c*. ſhew what the fuſiform Belemnite was, when perfect, with the alveolus *d*, *e*. Many of

[*r*] Theſe diſtinctions of the laminæ I preſume to be owing to the mineral ſteams inſinuating themſelves into the Belemnite, when the ſpar pervaded the pores, and deſtroyed the texture, but retained the true form by ſubſtituting its ſelf, and filling the Plaſm or mould of the Belemnite.

[*s*] Fig. 6. *b*.

 thoſe

thoſe found in chalk ſeem to be ſomewhat injured at the end *a*, where they are deficient, and are rounded, but have an uneven ſurface, as if they had been gnawed or eaten by the pholas. Thoſe found in clay near Piddington [*t*], Oxfordſhire, approach nearer to the fuſiform kind, and have a different appearance at the ſmaller end *f*, where the laminæ are reduced to a white impalpable powder, by corroſive juices in the earth, ſo as to ſtain the fingers when firſt taken out; and they afterwards retain a white chalky appearance: but, amongſt a great number, I never found one that was three inches long. Theſe have ſuffered in the ſame manner as Fig. iv. Fig. 3. *f*, ſhews where the alveolus terminates: *g*, *h*. *g*, *h*. how much has been deſtroyed by vitriolic acids [*u*]. At Stons-field they are found much longer than at Piddington, and are incloſed in ſtone, which is ſplit by the workmen to make ſlates. Here we often find

[*t*] Fig. 3.

[*u*] It may be aſked, why one part ſuffers more than another, as all parts are homogeneous, and free from extraneous mixtures? My anſwer is; becauſe thoſe parts, where the concamerated ſhell is lodged, are much thinner than the other parts of the Belemnite; and conſequently the walls are more eaſily broken down, and the alveolus, being ſtill leſs ſolid, is ſooner deſtroyed, and reduced to an impalpable powder, by vitrolic and other acids, which the water takes up as it paſſes through different ſtrata, abounding more or leſs with pyritical matter. Where no ſpar follows the acid, the parts are carried away and loſt in the interſtices of the earth, and a mould or plaſm is left, which Steno calls an aërial ſhell. See his Prodomus, pag. 84. But where the ſpar abounds, it pervades the whole ſubſtance, fills up the cavity, and aſſumes the true form of the ſhell; and ſometimes, by burſting the pores, is ſo far ſubſtituted in the place of the original particles, that the ſeveral diaphragms, with the ſiphunculus of the alveolus, are acurately and nicely preſerved.

them

them in a much more perfect ſtate [w] than the former, with the alveolus in many of them; but that part is commonly cruſhed [x] by the incumbent matter.

The ſiphunculus of the Belemnite is always upon the verge of the chamber, or cell; and in the ſiphunculus is a little gutt or ductus, proceeding from the body of the animal, by dilating or contracting of which the animal, it ſhould ſeem, may go out or into its cell at pleaſure. This is the only ſtay, which the animal has to ſecure its retreat: but I cannot agree with the learned doctor Hooke [z], *that the gut or ductus paſſes through all the cells to the end of the ſpiral cone*, either in this ſhell or the nautilus. His diſcovering of a ſpiramentum in the center of the latter was merely conjectural; for the ends of the ſpiral cone of concamerated ſhells [a] are ſhut up in the ſame manner with thoſe of the turbinated kind: and it is common for all turbinated ſhell-fiſh, as they increaſe in bulk, and enlarge their ſhells, to leave their bottom or firſt-formed convolutions. Therefore I make no doubt but the ſame is done by the concamerated tribe; for if the gut go through only one or two valves, it will be a ſufficient ſtay to the animal, and, being contracted or dilated, will ſerve all the purpoſes above mentioned. How far this is practicable by our little inhabitant, cannot abſolutely be determined; but if it be conſtantly fixed by the gut to the ſiphunculus, it has a ſurpriſing power of contracting and dilating its body, to extend ſo far as the bottom or point of the Belemnite,

[w] Fig. 2. [x] Ibid. at *y*.

[z] Hooke's poſthumous works publiſhed by Derham 8°. p. 306.

[a] See the little pearly cornu-ammonis ſhell.

which

which in ſome is more than thirty times the length of the cell, into which it returns [b]. I am apt to think, that this gut or ductus, as well as the body of the creature, is capable of being extended very conſiderably, to ſerve all the uſes of forming the Belemnite, without leaving the ſiphunculus; and that the gut ſerves for the ſame purpoſes with the tendons of the oyſter; the latter to open and ſhut the ſhell; the former to allow the animal to go out and in at pleaſure. And as the oiſter feeds altogether in the ſhell, by opening the verge, the Belemnite (whoſe reſidence is in the great deep, which is ſeldom diſturbed) very likely goes out in queſt of food, but travels only upon the guard or rampart, leaving a trail behind, as all land ſnails do; which hardening into a teſtaceous ſubſtance, increaſes the dimenſions of the outer walls, both in length and thickneſs, from the cell or chamber, to the bottom or point of the whole Belemnite. The animal in its progreſs and return claſps the whole guard, as a ſnail does a ſmall branch of a tree in the gardens; and where the two ſides meet, there the ſulcus is formed, as is evident from the laminæ in Fig. 9.

The Belemnites, like all other teſtaceous bodies, have the vermicular tribe attached to them, and are perforated by the pholades. Other marine bodies alſo affix themſelves to the Belemnites, oiſters in particular: but this never happens whilſt the animal inhabits the ſhell, becauſe the new additional laminæ would ſo cover the affixed body, and alſo the cells of the pholades and vermiculi, that they could have no communication with the water, and muſt conſequently

[b] Fig. 8.

 periſh.

perish. These bodies, thus attached, are the strongest proof we can desire, that the Belemnite is of marine production. Indeed it may be objected, that the bones of quadrupeds, wood, and stone have these bodies adhering to them, and therefore may be said to be marine, as well as the Belemnite. But when we bring them to chemical tryal, the objection vanishes; for the bones either come out of the furnace with a black core, or they are reduced to ashes; whereas the Belemnite is changed into a fine calx, after the manner of all testaceous bodies, and is converted into a species of phosphorus [c]. The oisters, having no loco-motion, frequently affix themselves to other bodies, that they may be better able to stem the tides, and currents, which might otherwise carry them from their proper beds, and places of feeding. This attachment to other bodies no way incommodes them, because they increase the dimensions of their shells by adding fresh laminæ inwardly: the first formed laminæ, being, as it were, excluded, lie in the manner of tiles upon the roof of a house, and exhibit the several steps or stages of their growth.

I believe a Belemnite is very rarely found perfect in the fossil state: those in gravel-beds [d] have suffered very much by being rubbed against stones, &c. by the fluctuating waters: those, which we find in rubble at Garsington-pitts [e], have many adventitious bodies adhering to them, and consequently were deserted by

[c] The Belemnite after calcination, has all the properties of the Bolognian stone. If it be exposed a few minutes to the Sun, and immediately taken into a dark room, it will shine like Phosphorus for some time; and when the light diminishes, if again exposed to the Sun, its splendor will be renewed.

[d] Fig. xv. [e] Fig. xvi.

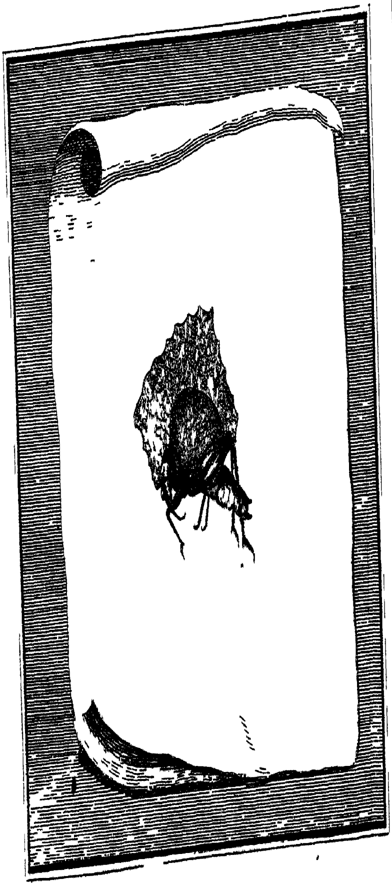
their inhabitants before they reſted there. In the clay at Shotover [*f*] near Oxford, they have a curious ſmooth ſurface, but are otherwiſe imperfect: at Stonsfield, in the ſlate-ſtone, they are generally cruſhed [*g*]: thoſe approaching neareſt to perfection, which I have ſeen, came out of the ſand [*h*], under the bed of ſtone at Garſington-pitts near Oxford: the outer part is quite perfect, and the verge of the conical cavity is as thin as paper, but the alveolus is deſtroyed, except the apex or point. At Thame, in digging for ſtone, ſeveral ſmall ones were found in a ſtratum of blue clay of a more cylindrical form [*i*]; ſome of which have the pearly ſubſtance ſtill remaining; an inconteſtable proof of their being marine productions.

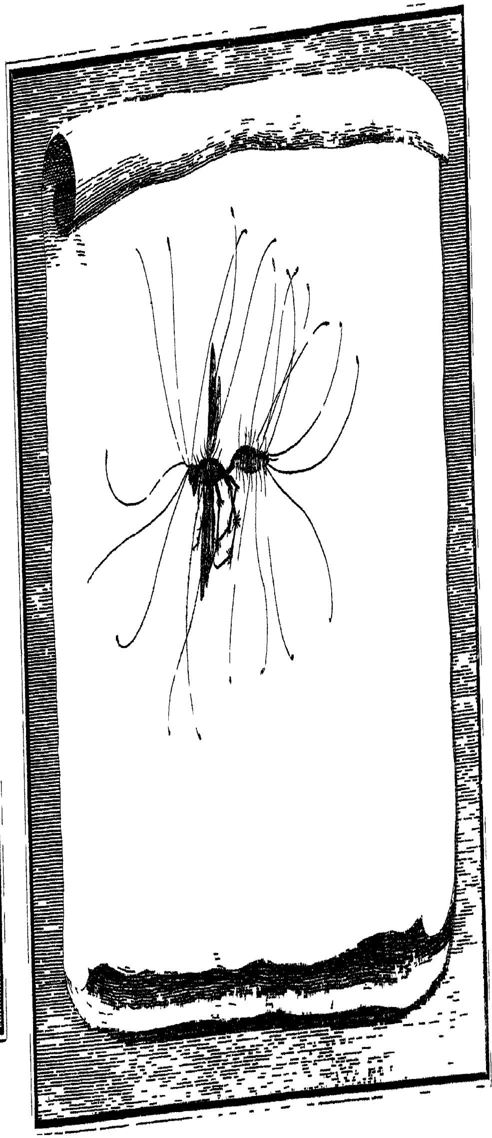
How much of the cavity is occupied by the alveolus, cannot be truly aſcertained, until a perfect one can be found, which it will be hard to do in the foſſil-ſtate; but if we may judge from the nautilus, the walls are carried to a diſtance from the laſt formed valve, much greater than that, at which the valves are placed from each other; as in fig. xiii. from *a*, to *b*, which gives the animal all the convenience of forming a new valve or diaphragm, *c*. This circumſtance has been very ingeniouſly cleared up by a learned phyſician in one of our monthly papers [*k*].

The recent nautili are very common in the eaſtern ſeas; and in the foſſil ſtate are frequently found with the Belemnites at Garſington near Oxford. Why may we not therefore expect to find a recent Belemnite,

[*f*] Fig. i. [*g*] Fig. 2. y.
[*h*] Fig. xiii. [*i*] Fig. xvii.
[*k*] Gent. Magazine for Jan. 1752. pag. 8.

as well as a recent nautilus, if a diligent perſon were ſtrictly to examine the coaſts, where the nautili are found? Perſons commiſſioned to collect ſhells, and other curioſities, generally graſp at ſuch things as feaſt the eye; not regarding thoſe of leſs beauty and luſtre, which would help us in our reſearches, and greatly illuſtrate the more uſeful parts of this ſtudy. A premium, offered by a Society of Vertuoſi, might encourage our ſea-faring gentlemen to ſearch the ſeveral coaſts, upon which they touch in their long voyages, and to uſe drag-nets as they do in drudging for oiſters: from ſuch aſſiſtance, variety of new ſubjects would be produced; and great improvements might be expected, if their inquiries ſhould be ſucceſsful.





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I. *An Account of a Singular Species of* Wasp *and* Locust: *By* Samuel Felton, *Esq; F. R. S. in a Letter to Mr.* Henry Baker, *F. R. S.*

Dear Sir,

King-Street, Covent-Garden,
December 2, 1763.

Read Feb. 2, 1764.

THE honour I received, by being elected a fellow of the Royal Society, excites me through gratitude to offer that learned body whatever occurs to me new, or worthy attention in the animal world; and the respect I bear you, dear sir, for your learning and goodness, to which I must add your having been so useful a member of that learned body such a number of years, and the encouragement you have constantly shewn towards promoting natural history, emboldens me to transmit to you this paper containing the descriptions of a very singular species of Wasp and of Locust, [TAB. VI.] which I met with in the Island of Jamaica. I made what search I could in the natural historians, but cannot find that they have ever been taken notice of, therefore are as yet unknown to the learned, or non-descripts. I therefore offer them by your means to the inspection of the Royal Society, to be inserted in the Transactions, if deemed worthy their attention. I beg leave to accompany these descriptions not only with the

subjects

ſubjećts themſelves, but alſo with accurate drawings to compleat their hiſtory, and am, with great eſteem,

Dear Sir,

Your moſt obedient

And moſt obliged humble ſervant

Samuel Felton.

I. CRINITA — Veſpa ſetis colli thoracis abdominisque radiantibus corpore longioribus.

Large as a common Waſp, but rather narrower.

The head is browniſh, the vertex black, in a triangular form.

The Antennæ are ſhorter than the thorax, a little thicker towards the end, of a yellow browniſh colour; but black in the middle.

The thorax is light browniſh on the back, but on the ſides and underneath black: before the inſertion of the wings, there are two yellow lines running tranſverſely downwards; juſt over the inſertion of the wings two hairs go out on each ſide of equal length, and very near twice as long as the whole body; from the upper part of the neck likewiſe go out two hairs as long as the body.

The Abdomen is divided into ſix ſegments of which the firſt is very narrow at its baſis, quite black, only the hind margins yellow; from this ſegment there only grow out two hairs twice as long as the abdomen, at the baſe but no where elſe; the other five

five segments are betwixt brown and yellow coloured, their hind margins a little paler, and the second has a black girth near the fore margin; hairs go out near the fore segment as rays; in the second only three, and they shorter than the abdomen, especially the side ones; in the third, fourth and fifth segments, there are four or five long hairs longer than the body, and several shorter ones, especially underneath where there are no longer ones; the sixth segment is terminated with a long hair.

All these hairs are of a light brown colour, seem to be stiff, but their ends are quite soft like papillæ, and from thence thicker.

The wings are shorter than the abdomen; the upper ones folded.

The legs are black, except the thighs which are yellow; at their joints there are short hair like rays, whose ends are likewise short and thickened.

II. Rhombea Cicada thorace compresso membranaceo foliaceo subrhombeo postico latiore.

The thorax is like a leaf that is raised perpendicularly from the body; it is three times as broad as the body, but the same length. This leaf is very near of a rhomboid figure, a little broader, or rather higher over the back; it is membranaceous, probably brownish, (when alive) half pellucid, with two spots that are more pellucid, or transparent; the larger one is very near the middle, but the smaller lower; the margins are waved, especially towards the hind angle; over the forepart of the body the leaf is double.

The

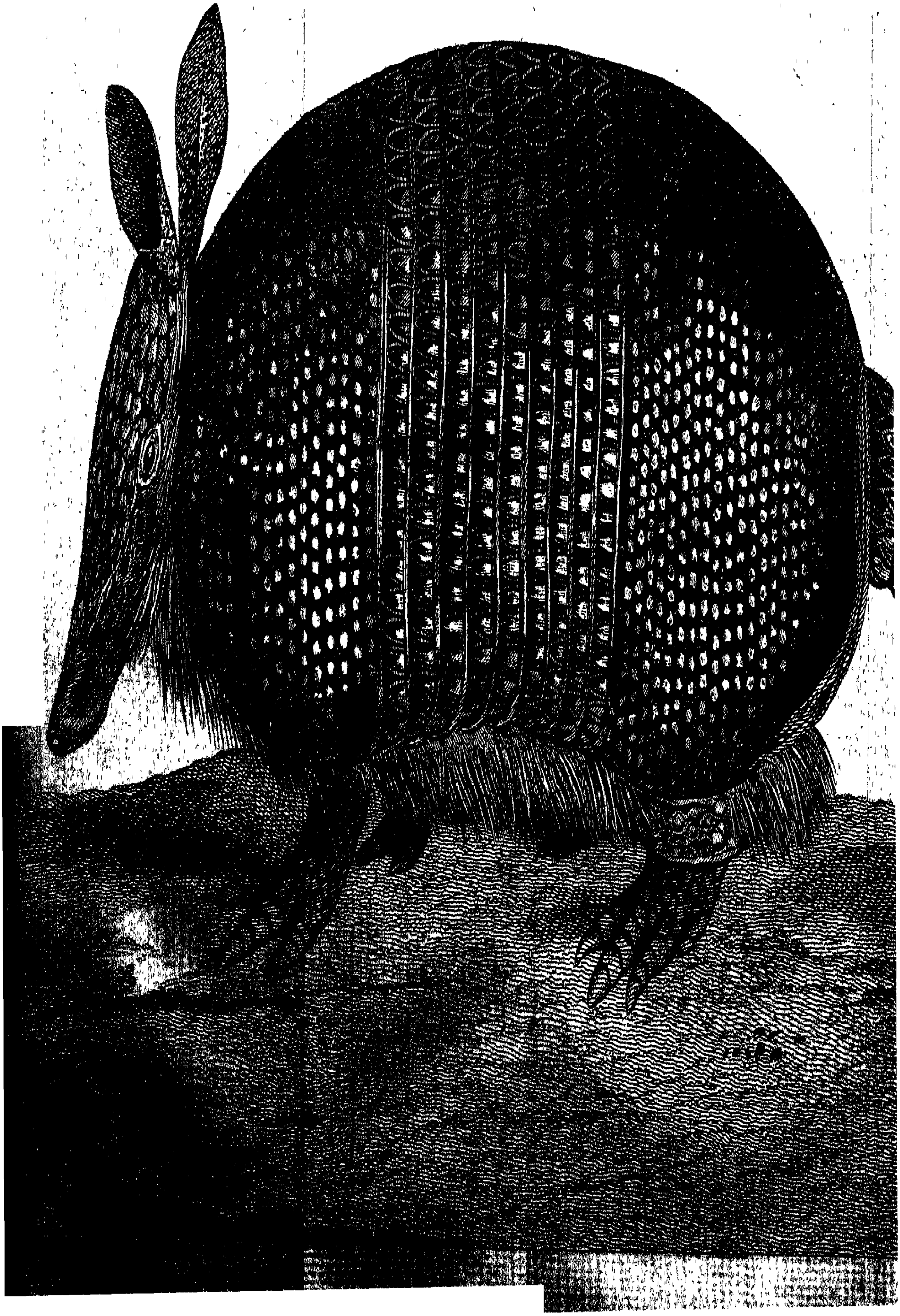
The abdomen is a little longer projected backwards than the leaf of the thorax.

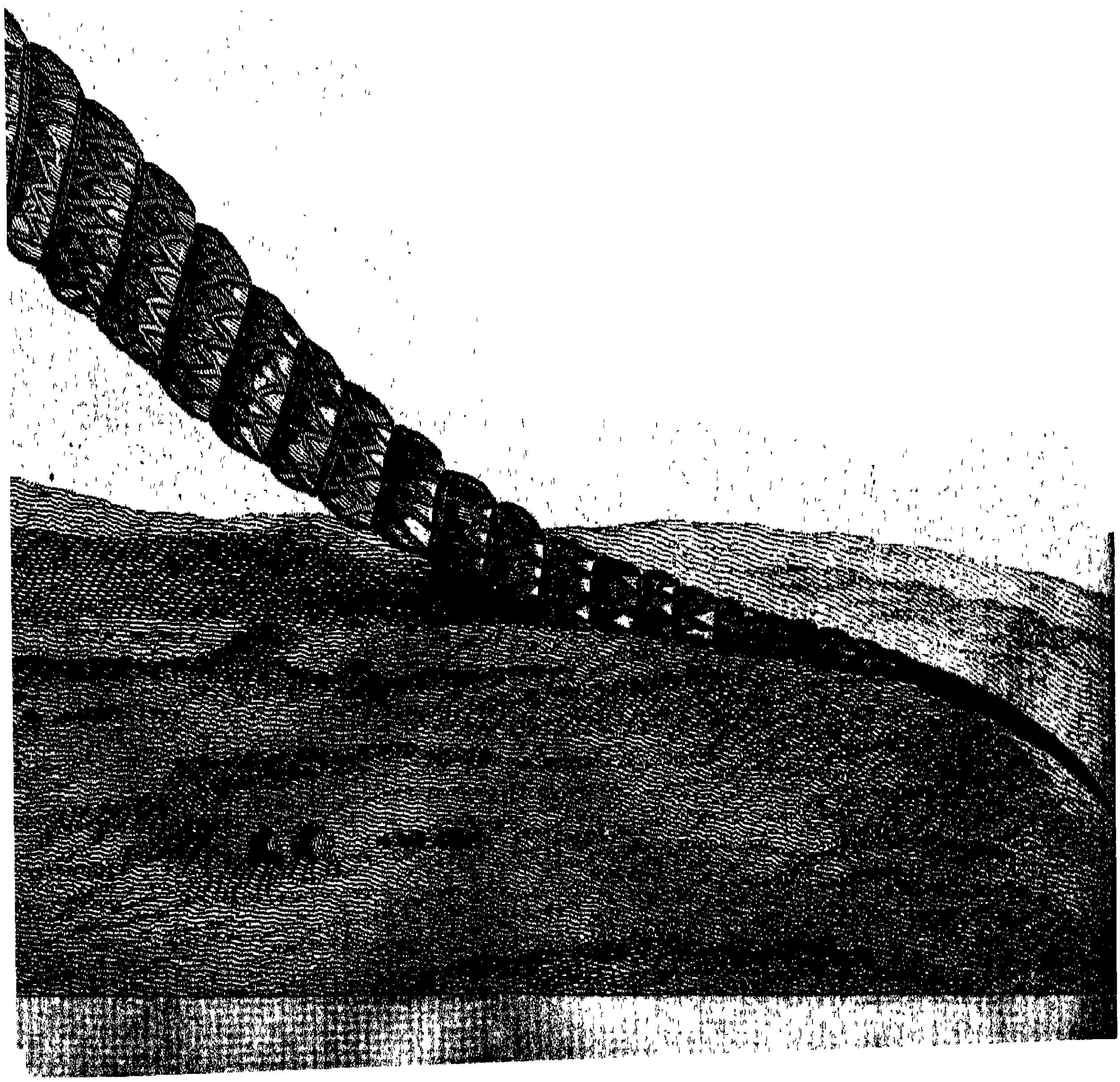
The insect had not yet got its coleoptera and wings.

The hind thighs that are thicker have on the upper side an additional narrow membrane added to them.

The head and maxillæ are very like those of the gryllus's, but there is such an affinity between this and the *Cicada foliata* Linn. syst. nat. 435.6. that I should think it the same species, if the thorax of this was not broader behind towards the end.

The antennæ are broke off; else from their length one might learn, to what genus the tribe Linnæus calls *Cicadæ foliaceæ* (syst. nat. p. 435.) should be referred; for I am in doubt whether Linnæus ever has seen perfect specimens of them.





VII. *An Account of an* American Armadilla: *By* William Watſon, *M. D. F. R. S.*

To the Royal Society.

Gentlemen,

Read Feb. 9, 1764.

I Herewith lay before you, for your inſpection, the drawing, by the ingenious Mr. Paillou, of an animal very ſeldom, if ever, ſeen alive in England [TAB. VII.]. It is now alive in excellent health, and in the poſſeſſion of the right honourable the Lord Southwell. It is called by Linnæus, in his Syſtema Naturæ, Daſypus cingulis novem, palmis tetradactylis, plantis pentadactylis.

Marcgrave and Ray have both deſcribed it under the appellation of Tatue Braſilienſicus. Albert Seba has likewiſe deſcribed it in the firſt volume of his elegant and elaborate Muſeum. He calls it Tatou, ſive Armadillus Americanus. The figure attending his deſcription is taken from a dead animal. The drawing therefore is hard and ſtiff, and the colouring does by no means come up to the living animal.

This creature, which is called by naturaliſts the American Armadilla, was brought hither a few months ſince to Lord Southwell, from the country near what is uſually called the Moſquito ſhore, upon the American continent. Its weight is ſeven pounds

 avoirdupois,

avoirdupois, and its ſize that of a common cat. It is a male, and has improved greatly both in appearance and colour, ſince it has been in his Lordſhip's poſſeſſion. It is fed with raw beef and milk, and refuſes our grain and fruits. In its own country, according to the accounts of thoſe who treat of it, it burrows in the ground.

As there is no good figure of this animal exiſting in any of the authors, who have treated of it; and as Lord Southwell has been ſo obliging as to permit me to have one taken of it by the before-mentioned able artiſt; I had reaſon to believe, that the inſpection of it would not be diſagreeable to the Society. I am, with all imaginable regard,

Gentlemen,

Feb. 9, 1764.

Your moſt obedient,

humble Servant.

W. Watſon.

VIII. *An Account of the Quantity of Rain fallen at* Mount's-Bay *in* Cornwall, *and of the Weather in that Place: In a Letter from the Rev.* William Borlase, *M. A. and F. R. S. to the Right Rev.* Charles *Lord Bishop of* Carlisle, *F. R. S.*

My Lord, Ludgvan, Oct. 31, 1763.

Read Feb. 16, 1764.

AT Carlisle, I find by your Lordship's note, there fell six inches and half of rain in the months of June and July last. In Mount's-Bay Cornwall, according to my ombrometer there fell

	In:	Tenths	Parts	inches	tenths	parts
In June — —	2	6	$\frac{1}{2}$	6	9	$\frac{1}{2}$
In July — —	4	3	0			

So that the rain in this part of Cornwall exceeded that at Carlisle almost half an inch. I could wish the gentleman at Carlisle would continue his observations, adding thereto a journal of Farenheidt's thermometer, and that we had another equally curious at Cathness, at least Aberdeen.

It is some amusement to compare the journal of the weather in one part with the accounts in the papers of storms, heats, and drought, and their contraries, in another.

On the 11th of August, there was at Brussels a most dreadful storm of thunder, lightning, and hail; at Ludgvan only misty-rain and showers.

On the 19th of the same month, when one of the most violent hurricanes ever known scourged some

 parts

parts of Kent from the West and South-West, it was calm, hazy, and sun-shine, and the wind at North-East, in Mount's-Bay, in the morning; in the evening South-South-East.

On the 2d of this month of Oct. there was a most violent storm on the Eastern coasts of Britain, from Yarmouth to Edinburgh; wind from the North-East and East-North-East; many ships distressed, many wrecked. What is remarkable, at the same time a like violent storm blew in the Western channel, along the coast of Cumberland, Lancashire, and Wales, but the wind from the West. In Mount's-Bay the wind was somewhat stormy and showery in the morning, the wind at West half North; in the afternoon windy and showery and sun-shine, West half South: You see how different, nay opposite, the winds, even in their extremest violence, are on the Eastern and Western coasts, where they have nothing between them but a narrow ridge of land. The cause of this remarkable opposition, I should be glad to see well explained. It must certainly have lain in the middle between the two forces; and it might contribute somewhat to the discovery, to know whence, and to what degree, the wind blew on the mountains in Scotland, and as far South as Derbyshire, from Sunday morning to Monday noon: but these are particularities not to be expected till the age becomes more philosophical. I remain, my Lord,

Your most obliged

and obedient Servant,

W. Borlase.

IX. *An*

IX. *An Account of a* Hernia *of the Urinary Bladder including a Stone: By Mr.* Percival Pott, *Surgeon to St.* Bartholomew's *Hospital, and F. R. S.*

Read Feb. 16, 1764.

A Healthy boy, about six years old, was suddenly seized with a most acute pain, at the bottom of his belly; during the time the pain lasted he could not discharge a drop of urine, tho' he frequently endeavoured. After about an hour and half, he became perfectly easy on a sudden, and pissed very freely.

A few days after this, a small tumor, about the size of a large pea, was discovered, in the upper part of the spermatic process, just below the groin. As this tumor was perfectly indolent, and gave the child no kind of uneasiness, no notice was taken of it. By slow degrees it descended lower and lower; and as it descended it seemed to increase in size: the boy was observed to make water oftener than usual, but without pain or difficulty. He was looked at by two or three practitioners in the country, who, not knowing what to make of it, advised the letting it alone; at last, in the space of five years, it got to the lower part of the scrotum; and, after it was got thither, it was observed to increase in size much faster than it had done before. The boy was at a great distance from London, and his friends could ill bear the expence of going thither with him; so that another year passed away after the tumor was got into the last-mentioned situation. At last, when he was about

thirteen

thirteen years old, the ſwelling becoming troubleſome, and the people in the country not caring to meddle with it, he was brought to London.

Two or three gentlemen of the profeſſion, to whom he was ſhowed, took it for a ſchirrhous teſticle, and adviſed the extirpation of it; to which the child's friends would not conſent.

When he was brought to me, I examined him very carefully, and was ſatisfied that the tumor, (which was now about as big as a middling cheſnut) was not formed by the teſticle: but, though I was clear that it was not formed by that gland, yet I could not find any teſtis on that ſide.

The ſwelling was ſtill perfectly void of pain; had a ſtony, incompreſſible, hardneſs; was troubleſome to the child when at play or uſing any briſk exerciſe, but never gave him any uneaſineſs when he ſat, or ſtood ſtill. It had all the appearance of being dependent from the ſpermatic proceſs; but the proceſs, tho' it had neither the look nor the feel of being diſeaſed, was yet too large, and too full for a child of that age, and larger and fuller than that of the other ſide. The perfect equality and ſmoothneſs of the tumor, its extreme, incompreſſibility and its being perfectly free from pain, even when preſſed with ſome force, were the circumſtances which induced me to believe that it was not the teſticle; but, tho' I was in my own mind ſatisfied of that, yet I cannot ſay that I was by any means clear what it was; and all that I could determine, was, that it certainly ought to be removed; as well on account of the trouble it now gave, and its manifeſt diſpoſition to increaſe,

increaſe, as that I could not foreſee any great hazard that was likely to attend its extirpation.

From the uncertainty in which I was concerning the true nature of the caſe, I determined to act very cautiouſly. I made an inciſion thro' the ſkin and cellular membrane, from the upper part of the ſcrotum quite down to the lower; by which I diſcovered a firm, ſtrong, white membraneous ciſt, or bag, connected looſely with the ſkin by means of the dartos; I diſſected all the anterior part of this ciſt, quite clean; and found, that, as I traced it upward, it became narrower, and ſeemed to proceed from the groin: This determined me to try if I could not free the poſterior part of it ~~alſo.~~ In doing this I diſcovered the teſticle, which was much compreſſed, flat, very ſmall, and lay immediately behind the tumor.

The diſſection of the teſticle and of the ſpermatic chord from the bag, and from its neck (which I was obliged to do in order to preſerve the teſtis) took up ſome time, and gave me ſome trouble; but, when I had finiſhed it, I found that the ciſt was dependent from, or continuous with, a membranous tube, or duct, of about the breadth of a large wheat ſtraw, which ſeemed to paſs out from the abdomen, thro' the opening in the oblique muſcle, along with the ſpermatic veſſels.

When I had perfectly freed this duct from all connexion, I cut it thro' immediately above the tumor; upon the diviſion of it a quantity of limpid fluid (not leſs than two ounces) followed, and the mouth of the ciſt expanding itſelf diſcovered a large ſtone, exactly reſembling the calculi found in the urinary bladder;

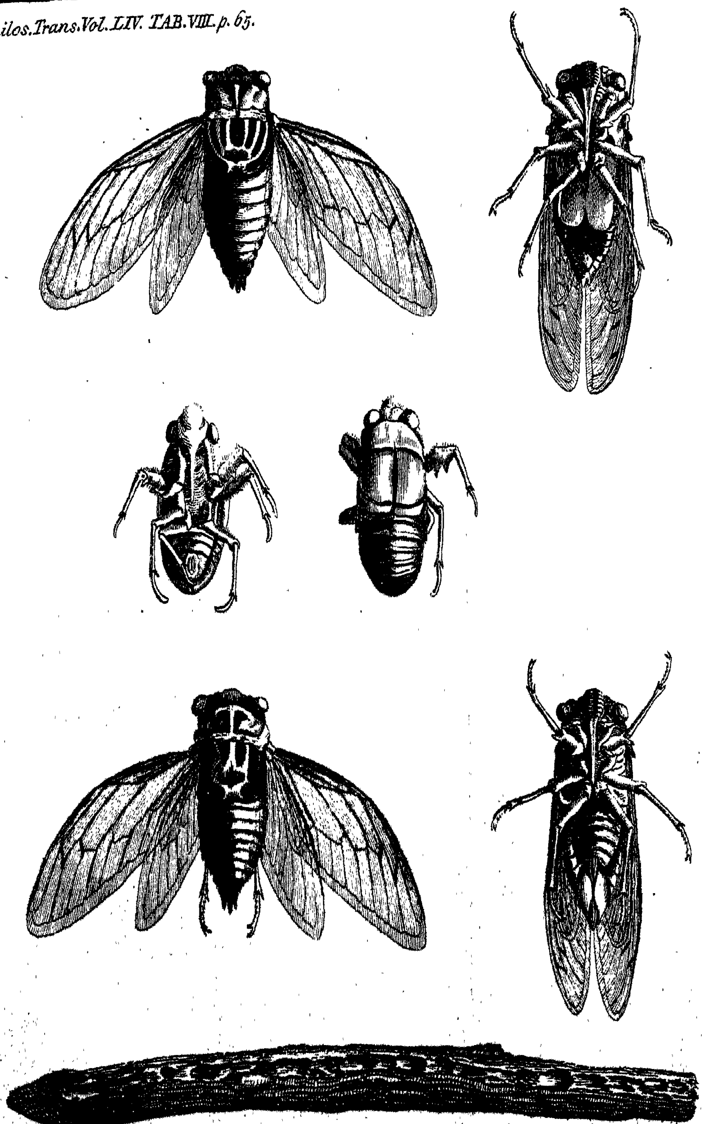
bladder; which ſtone was cloſely embraced by the ſaid ciſt.

As there was not the leaſt appearance of any fluid either in the bag or duct, before it was cut off, this diſcharge, together with the ſtone, induced me to ſuſpect that the caſe was a Hernia ciſtica. In order to be certain, I ſtaid ſome time; and, when I thought it was probable that ſome urine was derived into the bladder, I deſired the boy to make water; he endeavoured ſo to do, and a full ſtream of urine flowed out through the wound in the groin, which put the caſe beyond all doubt.

I dreſſed him ſuperficially; he had no bad ſymptom; his urine all paſſed out by his wound for a fortnight, or twenty days; at the end of which time, the wound gradually contracted; all the urine came through the urethra; and at the end of a month he was perfectly well.

P. Pott.

Philos. Trans. Vol. LIV. TAB. VIII. p. 65.



J. Mynde sc.

X. *Some Observations on the* Cicada *of* North America, *Collected by Mr.* P. Collinson, *F. R. S.*

London Nov. 2, 1763.

Read Feb. 23, 1764. IN Pennsylvania the Cicada is seen annually, but not in such numbers as to be remarkable; but at certain periods, of 14 or 15 years distance, they come forth in such great swarms, that the people have given them the name of *Locusts.* About the latter end of April these Cicadæ come near the surface: this is known, by the hogs routing after them. They creep out of the ground, near the roots of trees, in such numbers, that in some places, the earth is so full of holes, it is like an honey-comb.

Their first appearance is an hexapode (an ill-shapen grub) with six feet. This is their middle or nymph state: they creep up every thing near them, and fix their claws fast, on the shrubs, and bark of trees: then the skin on its back bursts open, and the fly comes forth, disengaging itself by degrees, leaving the case or exuviæ behind, in the exact shape, in which it was before occupied.

At first coming out, the Cicadæ are all white, with red eyes, and seem weak, and tender; but next day they attain to their full strength and perfection, being of a dark brown colour, with four finely-veined transparent wings, as will be better seen, than described, by the specimens before you. [TAB. VIII.]

 They

They come forth out of the ground in the night; being then ſecure from being diſturbed by ſo many creatures, that prey on them, whilſt they are under the operation of exchanging one ſtate for another. From the tenth of May to the fifteenth, they are obſerved to be ſpread all over the country.

As ſoon as the dew is exhaled, the Cicadæ are very active, flying about from tree to tree. The male makes a ſinging noiſe, calling the female, which he effects by a tremulous motion he gives to two bladders, filled with air, under his wings. From their numbers the noiſe is ſo loud and troubleſome, that it interrupts converſation with a continual dinn, from morning to evening. They continue coupling to the ſixteenth of May: ſoon after the males diſappear, and the females lay their eggs. They are much larger than the males.

They never could be perceived eating any thing; yet, as they are furniſhed with a long proboſcis, which they frequently extend, they may ſuck the dews, or the farina of flowers.

The male, in coupling, hath, at the end of his tail, two hooks, with which he enters between the rings, that ſurround the body of the female. Theſe, ſpreading internally, confine them long together; which may be requiſite, as there is a great number of eggs to impregnate, ſome ſay ſix or ſeven hundred.

Soon after this work is over, the female begins laying her eggs. To aſſiſt her in this operation, ſhe is armed with a dart near half an inch long, fixed between her breaſt and belly, and which extends to the end

of

of her tail. This ſhe ſheaths up, when it is not in uſe: with this dart ſhe pierces the ſmall twigs of trees, and, at the ſame time, injects an egg. The darted twiggs, that lie before you, will better ſhew the manner, than I can deſcribe it.

It is ſurpriſing to ſee how quick they penetrate into hard wood, and croud it full of eggs, the length of two or three inches, ranged in a line cloſe together, from twelve to eighteen in each partition. How ſhe depoſites the eggs in this direction, it was difficult to diſcover, they are ſo very ſhy whilſt about this work: but my ingenious friend John Bartram, obſerving her, in the beginning of this operation, took a ſtrong woody ſtalk of a plant, and, preſenting it to her, ſhe directly fell to work upon it, as he held it in his hand. It was very wonderful to ſee how dextreouſly ſhe worked her dart into the ſtalk, at every puncture dropping an egg. This was ſeen very diſtinctly, as ſhe did not touch the ſtalk with any other part of her body.

The Cicadæ fix on moſt ſort of trees, but like beſt the oak and cheſnut, which are the twigs before you, and the ſaſſafras, and all orchard trees.

They always dart to the pith of the branch, that, when the egg hatcheth, the little inſect may find ſoft food in its infant ſtate. When mature, they creep forth, go down the tree, or drop off, and ſoon make their way into the ground, where they have been found two feet deep. Here they find a ſecure repoſe, untill they have paſſed through their changes, from a magot to an hexapode, and laſtly to a fly.

July 15th and 16th they were perceived coming forth: ſeveral darted twigs were perceived, and care-

fully examined, and opened: ſome eggs were hatched, others not mature, of a dull brown colour. Theſe were taken out, and ſpread on a table; in about an hour the eggs cracked. It was very entertaining to obſerve, how the little inſect contrived to diſengage itſelf, from the ſhell. When it was got clear from its incumbrances, it run about, very briſkly, ſeeking a repoſitory in the earth.

Some General Remarks.

Theſe Cicadæ are ſpread all over the country in a few days; but, being the prey of ſo many animals, their numbers ſoon decreaſe, and, their duration by the order of nature being ſhort, quickly diſappear.

They are the food of moſt kind of domeſtic and wild fowl, and many beaſts: even the ſquirils grow fat with feeding on them.

And one of the repaſts of the Indians, after having firſt plucked off their wings, is to boil and eat them.

There are two diſtinct ſpecies of Cicadæ in North America; the one here deſcribed being much larger than the other.

The leſſer ſpecies has a black body, with golden eyes, and remarkable yellow veined wings.

XI. *An*

XI. *An Account of the Plague at* Conftantinople: *In a Letter from* Mordach Mackenzie, *M. D. to Sir* James Porter, *His Majefty's Envoy Plenipotentiary at* Bruffels, *and F. R. S.*

S I R,

Read Feb. 23, 1764.

SO many great men have written upon the Plague already, as Profper Alpinus, Sydenham, Hodges, Diemerbroeck, Muratori, Mead, &c. that it might be juftly thought prefumptuous in me to touch upon that fubject after them. But as I find, that they differ in fome circumftances, and that fome of them have had an opportunity of feeing only one year's plague; I may be allowed to write to you fuch remarks, as I have made for almoft thirty years, that I have lived in this plaguy country, without any quotations or confirmations from other authors; which I hope will help to reconcile the different opinions of the above-mentioned famous authors. Which tafk I would choofe rather, than to contradict them; for I am perfuaded, that each of them wrote according to the beft of his knowledge (as I do myfelf) without any intention of impofing in the leaft upon mankind.

It is beyond difpute, that the plague appears in a different manner in different countries; and that it appears differently in the fame country in different years: for we find moft other difeafes alter more or lefs.

leſs, according to the conſtitution and diſpoſition of the air in the ſame climate: for, ſome years, fevers are epidemic, and very mortal: other years, they are epidemic, but not mortal; the ſmall pox the ſame; &c. And ſo the plague is ſome years more violent, and has ſome ſymptoms different from what it has in other years; which, I take for granted, muſt be the reaſon of any difference, that may appear in the remarks of the celebrated authors already mentioned. There is one extraordinary ſymptom, which the moſt of theſe authors mention, tho' none of them prove it, or pretend to have ſeen it; which ſeems to me inconſiſtent and incompatible with the animal œconomy; making ſtill proper allowance for Omnipotence and Divine Vengeance, as in that of Sennacherib's numerous army, and many other ſuch plagues, mentioned in Scripture. What I mean, is, that a perſon cannot die of the plague (ſuch as it appears among us) inſtantaneouſly, or in a few hours, or even the ſame day, that he receives the infection. For, you know, Sir, by your long experience in this country, that all ſuch, as have the plague, conceal it as long as they can, and walk about as long as poſſible. And I preſume it muſt be the ſame in all countries, for the ſame reaſon, which is the fear of being abandoned and left alone; and ſo, when they ſtruggle for many days againſt it, and at laſt tumble down in the ſtreet, and die ſuddenly, people imagine, that they were then only infected, and that they died inſtantly of the infection; tho' it may be ſuppoſed, according to the rules of the animal œconomy, that the noxious effluvia muſt have been for ſome time mixed with the blood, before they could produce a fever, and afterwards that corruption

and putrefaction in the blood and other fluids, as at last stops their circulation, and the patients die. This was the case of the Greek, who spoke with your master of horse, Knightkin, at the window, anno 1752, and went and died in an hour afterwards in the vineyards of Buiuk deré; and it was said he died suddenly, tho' it was very well known to many, that he had the plague upon him for many days before this accident happened.

Mrs. Chapouis found herself indisposed for many days, anno 1758, and complained pretty much, before she was suspected to have the plauge. Captain Hills' sailor was infected in Candia 1736; was a fortnight in his passage to Smyrna, as the captain swore to me; yet he was five days in the hospital there before he died. Mr. Lisle's gardiner was indisposed twelve days before he took to his bed, and he lay in bed eight days before he died, in July 1745.

It is true, that Thucydides, in his account of the plague at Athens, relates, that some were said to die suddenly of it; which may have led others into the same way of thinking: but Thucydides (with all due regard to him) must be allowed to have known very little of the animal œconomy, for he was no physician, tho' a very famous historian; and he owns moreover, that, when the plague first attacked the Piræeum, they were so much strangers to it at Athens, that they imagined the Lacedæmonians, who then besieged them, had poisoned their wells, and that such was the cause of their death. Besides, he pretends to affirm, from the little experience he had of the plague, that the same person cannot have it twice, which

which is abſolutely falſe. The Greek Padré, who took care of the Greek-hoſpital at Smyrna for fifty years, aſſured me, that he had had the plague twelve different times in that interval; and it is very certain, that he died of it in 1736. Monſieur Broſſard had it in the year 1745, when he returned from France; and it is very well known, that he and all his family died of it in April 1762. The Abbé, who takes care of the Frank-hoſpital at Pera, ſwore to me the other day, that he has had it already, here and at Smyrna, four different times. But, what is ſtill more extraordinary, is, that a young woman, who had it in September laſt, with it's moſt pathognomonic ſymptoms, as buboes and carbuncles, after a fever, had it again on the 11th of April, and died of it ſome days ago, while there is not the leaſt ſurmiſe of any accident in or about Conſtantinople ſince December, this only one excepted: but there died four perſons in the ſame little houſe in September; and as the houſe was never well cleaned, and this young woman always lived in it, ſhe was at laſt attacked a ſecond time, and died.

The only antecedents, that I could obſerve to this malady, was a great murrain among the black cattle in May 1745, and in the beginning of June, the ſame year, ſwarms of butterflies flew about, and there were great numbers of caterpillars creeping every where, and afterwards a violent plague: and, after obſerving the ſame anno 1752 and 1758, you may recollect, that I foretold to you, Sir, that we ſhould have a hot plague in thoſe years; which accordingly happened, eſpecially in the months of Auguſt and September 1758, when many of Marſellini's family, Spathari,

Spathari, Skwackhim's cook, Charlacci Rimbeault, Jackino's ſon, &c. died of it.

The plague is now more frequent in the Levant, than it was, when I came firſt into this country, about 30 years ago; for then, they were almoſt ſtrangers to it in Aleppo and in Tripoli of Syria, and they had it but ſeldom at Smyrna; whereas now they have it frequently at Aleppo, and ſummer and winter in Smyrna, tho' never ſo violently in the winter; which muſt be owing to the great communication by commerce over all the Levant, and more extended into the country villages than it uſed to be. I take the plague to be an infection communicated by contact from one body to another; that is, to a ſound body from an infected one, whoſe poiſonous effluvia, ſubtile miaſmata, and volatile ſteams, enter the cutaneous ſores of ſound perſons within their reach, or mix with the air, which they draw in reſpiration, and ſo advancing by the vaſa inhalantia, mix with the blood and animal fluids, in which, by their noxious and active qualities, they increaſe their motion and velocity, and in ſome days produce a fever; ſo that the nearer and the more frequent the contact is, the greater is the danger, as the noxious particles, exhaling from the infected perſon, muſt be more numerous, and conſequently have greater force and activity in proportion to their diſtance.

Some perſons are of opinion, that the air muſt be infected, and that it is the principal cauſe of theſe plagues; whereas I preſume, that the ambient air is not otherwiſe concerned, than as the vehicle, which conveys the venemous particles from one body into another, at leaſt in ſuch plagues, as I have ſeen hitherto

 at

at Smyrna and Conſtantinople; allowing always, that the different conſtitution of the air contributes very much to propagate the plague: for the hot air dilates and renders more volatile and active the venemous ſteams, whereas cold air contracts and mortifies them. The perſon having the plague may be ſaid to have a contagious and poiſonous air in his room and about him, while at the ſame time the open air is free from any dangerous exhalations; ſo that I never was afraid to go into any large houſe, wherein a plaguy perſon lived, provided that he was confined to one room.

The peſtilential fever ſhews itſelf firſt, by a chilli-neſs and ſhiverings even in the months of July and Auguſt, ſo very like the firſt approaches of an ague, that it is impoſſible to diſtinguiſh the one from the other at firſt ſight. This cold fit is ſoon accompanied with a loathing nauſea and deſire of vomiting, which obliges the patient at laſt to diſcharge a vaſt quantity of bilious matter, with great uneaſineſs and oppreſſion in the thorax and mouth of the ſtomach, attended ſometimes with a dry cough, as in an intermitting fever; and even in this ſtage it is very difficult to diſtinguiſh the one from the other. Next, the patient has a violent head-ach and giddineſs, with ſome ſlight convulſive motions: he breathes hard; his breath and ſweat ſtink; his eyes are ruddy, he looks frighted, ſad, and pale; he has an inſatiable thirſt; his tongue is yellowiſh, with a red border; he has a total loſs of appetite, reſtleſſneſs, great inward heat, and more than could be expected from the fever, which is ſometimes pretty moderate, but grows ſtronger frequently towards night: the patient very often bleeds

at the noſe. He continues in that diſmal condition for ſome days, untill the venemous matter begins to be ſeparated in ſome meaſure from the blood, and diſcharge itſelf critically upon the ſurface by the cutaneous eruptions of buboes, carbuncles, blains, petechial ſpots, and ſome ſmall veſicles or bliſters: but all theſe ſymptoms are not to be looked for in the ſame perſon. When the cutaneous eruptions appear and grow ſenſibly, the patient finds himſelf better, and ſomewhat relieved from the great oppreſſion he laboured under before. Some perſons in the above ſtate have a very violent fever, ſometimes attended with a delirium and phrenzy; others are ſtupid, ſleepy, and complain of nothing: as one of Captain Hill's men mentioned before; and the young fellow, who died of the plague laſt year, which he had in our palace; for I no ſooner found, that he had a fever, and was at the ſame time ſo very ſtupid and ſenſeleſs, but I concluded he had the plague, tho' it was ſtrenuouſly maintained by the ſervants, that he had not been out of the kitchen for a month; but, upon ſtrict examination, it was found, that he had many plaguy ſymptoms, as buboes, carbuncles, &c. upon his body, and that he had been in an infected houſe near the palace, about 12 days before; wherein no doubt he received the infection. Such as are furious and delirious, ſeldom live ſo long, as they who are ſleepy and ſtupid; but if they live long enough to have the cutaneous eruptions puſh plentifully, and their phrenzy begins to abate afterwards, they may recover more probably than ſuch as are ſleepy and have a moderate fever; tho' I have known ſome of them likewiſe die; as Delaria, the French druggerman,

who went on horſe-back on Friday to Giamdeié, looking upon himſelf paſt danger, but died next Saturday morning. Marſellini's eldeſt ſon, 1758, thought himſelf ſo very well after the eruptions of the buboes, that he went from town, and dined at Therapea, and returned to town the ſame evening and died, after he had been delirious for ſome days before, and had had the plaguy fever from the time he left Buiukderé about ten days before.

I make no doubt, Sir, but you are very ſenſible, that nothing in this country, either air or diet, produces the plague, tho' both contribute very much to it's progreſs and violence, after it is brought here or to any part of this country from any other infected place; for you know, by long experience, that it rages moſt in the hot months of July, Auguſt, and September, when the diet of moſt of the poor inhabitants (who are the greateſt ſufferers by the plague) conſiſts of unripe fruits, cucumbers, melons, gourds, grapes, &c.

The plague breaks out here and at Smyrna ſome years, when it is not poſſible to trace whence it is conveyed; for ſome houſes, which were infected, and not well cleaned after the infected perſon is removed, lodge ſome of the venemous moleculæ in wool, cotton, hair, leather or ſkins, &c. all winter long; which, put in motion by the heat in April or May, breathe out of their nidus, where they reſided, and recover ſo much life and action, as to enter into the cutaneous pores of any perſon, who comes withih their reach, and ſo infect him; as it happened at the French palace, at Mr. Hubſch's and at Caraja's houſe, for two or three years running. But plagues of this kind

ſeldom ſpread, and are never ſo fatal, as ſuch as come from abroad.

Many are of opinion, that the heat kills the plague, as they term it, which is owing to a fooliſh ſuperſtition among the Greeks, who pretend, that it muſt ceaſe the 24th of June, being St. John's day, tho' they may obſerve the contrary happen every year; and the ſtrongeſt plague, that was at Smyrna in my time, anno 1736, was hotteſt about that time, and continued with great violence till the latter end of September, when it began to abate; but was not entirely over till the 12th of November, when Te Deum was ſung in the Capuchins convent.

This miſtaken notion may be in ſome meaſure owing to a wrong ſenſe put upon Proſper Alpinus, who allows that the plague at Cairo begins to ceaſe in the months of June and July, when the ſtrong Northerly winds (called Embats or Eteſian winds) begin to blow, which make the country much cooler than in the months of May, April, and March, when the plague rages moſt; which he very juſtly imputes to the great ſuffocating heats and Southerly winds, which reign during thoſe months in that country: and it is then, that the ſhips, which load rice, flax, and other goods and merchandiſe for Conſtantinople receive the infection, and carry it with them hither; and, upon theſe goods being delivered to perſons in different parts of the city, the plague breaks out at once with great violence among the trading people of the Greeks, Armenians, and Jews; for I have obſerved, both here and at Smyrna, that the Turks are commonly the laſt of the four nations, who are infected; but when the plague gets once among them, they ſuffer

fer moſt by it, becauſe they take the leaſt care and precaution, and their families are much more numerous.

The plague, as well as all other epidemical diſeaſes, has it's riſe, progreſs, ſtate, and declenſion, when it begins to loſe it's virulence, and many of the ſick recover. Some years it is felt ſporadically all the winter; and we hear ſome accidents in the Phanar, among the Greeks, among the Jews, Turks, and Armenians; and even among the Franks; for you may remember, that Pera was not clean all the winter 1762. Some years it lodges in the villages upon the Boſphorus; but during the winter it is never of any great conſequence.

As to the cure of this diſeaſe, ſome are for bleeding plentifully, as Leonardus Botallus and Doctor Dover, &c. But in this country, it is reckoned infallible death to open a vein, and therefore bleeding is never uſed: But I am of opinion that a medium between theſe two extremes might prove more to the purpoſe; for, as it is an inflammatory diſeaſe, bleeding and emetics might be of uſe in the beginning, as ſoon as the patient is taken with the fever, eſpecially if the fever is very hot and attended with a delirium or any violent head-ach; but after there begins a ſeparation of the morbific matter, which the ſtrength of nature, and the agitation of the fever, drive upon the ſurface of the body in buboes or carbuncles, bleeding or purging muſt prove very prejudicial; but gentle vomits might be of ſervice even then, as they might drive out thoſe cutaneous eruptions more powerfully than nature could do it without any help. The vomits likewiſe might prevent the return of the

morbific

morbific matter into the blood, which frequently happens, and the buboes, &c. disappear, and the patient infallibly dies in a very short time. As the pestilential fever has many remissions, I am of opinion, that the use of the bark in the remissions might be of great service; as it proved anno 1752, when the French ambassador's servant was saved at Buiukderé, by means of some bark and ipecacuana, which I sent with directions to Padré Joseppé; and he was the only person, that recovered of all the gang, who were then taken ill in our village.

The practice in the hospital is after this manner: when any person is suspected, they give him a large dose of brandy with a dram of Venice treacle; and afterwards they cover him very well that he may sweat: for the first three days, he eats nothing but vermicelli boil'd in water, with a little lemon juice. On the fourth day they give him rice and water; which diet they observe strictly till the 15th or 20th day, when they begin to allow him very thin chicken broth, commonly called brodo longo, and they give him from first to last nothing but warm water to drink.

They apply first to the buboes and parotides a cataplasm of mallows and hog's lard, to advance maturation; and, after they are ripe and open, they dress them with basilicon ointment.

They apply caimack and sugar to the carbuncles for some days to cool them; and when they begin to separate, they apply a digestive of Chio turpentine with the yolk of an egg. They apply nothing to the blains and petechial spots, which appear and disappear again upon any part of the body every three or four days.

All this time they give the ſick no medicines, beſides Venice treacle for the poor, and ſome doſes of bezoar for ſuch as can afford to pay for it; and they never can be perſuaded to change their method; for when you gave them Doctor James's powder, they never tried what effect it might have.

I am of opinion that all antiphlogiſtics ſhould be uſed before the eruptions; and all alexipharmics and antiſeptics after them; more particularly camphire, and ſome doſes of bark always in the remiſſions of the fever, and bliſters ought to be of great uſe in the ſleepy and ſtupid plague, for rouſing the animal ſpirits, and for giving them ſome motion: but they are never uſed here; and, as they live by cuſtom, it is impoſſible to prevail upon them to change it.

As to preſervatives, I think the beſt is to remove from the infected perſons and houſes, and to keep at a proper diſtance for many days from them.

Some are of opinion, that fire preſerves from the plague, and purges the air; from whom I beg leave to differ; for I have remarked here, that cooks and cooks mates, who are always near the fire, ſuffer more by the plague, than any other ſet of people in proportion to their number. Beſides, the fire enlivens and gives energy to the poiſonous effluvia lodged about them, which otherwiſe might die and diſperſe in the open air, if expoſed ſufficiently to it. Fire moreover opens the pores, relaxes the fibres; and, as the hot weather propagates the plague, fire ſhould do the ſame more or leſs; and for the ſame reaſon I imagine, that all perfumes muſt be of very little ſervice.

The next beſt preſervative I take to be moderation, and a diet of ſuch meats as are of eaſy digeſtion,

tion, of a rich balſamic quality, and capable of producing a rich and generous blood. It is likewiſe a great preſervative to be under no apprehenſion, and to guard as much as poſſible againſt diſmal thoughts and imaginations upon ſuch occaſions.

Thus, Sir, I have laid before you, in a few words, all that occurs to me upon the ſubject; and as moſt of the examples related happened in your own time, and are conſiſtent with your memory and knowledge, I hope you can bear witneſs to the truth of the facts, if you think proper to preſent them to the Royal Society; and if not, you may diſpoſe of them as you pleaſe, for I wrote them in obedience to your deſire, and to give you an evident proof of the profound reſpect, with which I am,

Sir,

Conſtantinople,
St. George's day,
1763.

Your moſt obedient

humble ſervant,

Mordach Mackenzie.

1748. The plague began the 10th of May, and ended in November.

1749. It began the 16th of March, and ended the 20th of October.

1750. It began April 21ſt, and ended the 17th of September.

1751. It began the 15th of May, and continued all the ſummer, autumn, winter, and to the latter end of September 1752.

1753. It began May 31ſt, continued all the ſummer, autumn, winter, and till the 17th of September 1754.

1755. It began in June, but there was very little plague all this year.

1756. It began March the 6th, and ended the 12th of December.

1758. Then there was none till the 23d of April 1758, which ended in October.

1759. It began April 4th and ended about the 10th of September.

1760. It began April the 24th, and ended the 10th of November.

1761. It began the 10th of March, and continued till the 19th of December 1762. Since which day there has not been hitherto one accident, beſides that of the young woman on the 11th of this month already mentioned.

In 1751, the 20th of October O.S. a vaſt quantity of ſnow fell, that cut off the diſtemper, and there was little plague in 1752. The former year was the moſt conſiderable, and more univerſally mortal at Conſtantinople than any in the ſpace of fifteen years.

XII. *An*

XII. *An Account of a remarkable Tide at* Bristol: *In a Letter to the Rev.* Thomas Birch, *D. D. Secret. R. S. from the Rev.* Josiah Tucker, *D. D. Dean of* Gloucester.

Read Feb. 23, 1764.

ON Saturday the 11th instant, when the tide had hardly begun to flow, according to it's regular course, it was observed, by the water-bailif of the city, and by several others, both on the back, and at the key, to rise very suddenly to almost high-water mark; and it so continued for near half an hour: then it sunk, almost instantaneously, three feet perpendicular: after that, it began to flow in again, and kept flowing on till one of the clock, and rose to the height it was expected to do.

At Rownham Passage, a mile below the city, the ferry-men observed the tide to ebb almost instantaneously, and to sink at least four feet perpendicular. Then it flowed in again, as it should have regularly done.

Ar King-Road, which is about three miles below the city, the officers observed the king's boat to float suddenly, which they attributed to a great fresh comming. But they found afterwards the boat presently aground.

I could get no intelligence of any thing observable, that happened in the river Severn, excepting that at Gloucester, and at Worcester, the inundation sunk very

very faſt on that day. But moſt undoubtedly the ſtrong rapid tide of the Severn muſt have been affected in a very remarkable manner, had there been any curious perſons to take notice of it.

XIII. *A Letter containing ſome Experiments in Electricity, to Mr.* Benjamin Wilſon, *F. R. S. from Mr.* Torbern Bergman, *of* Upſal, *in* Sweden.

Ampliſſime atque celeberrime domine,

Read Feb. 23, 1764.

EXperimentorum circa corporum adfrictum jam ea ſunt commemoranda, quæ cum ſerico inſtitui; cujus vis electrica, etſi a domino Gray dudum inventa, tamen hucuſque parum fuit explorata. Pulchra ſunt quæ adtulit dominus Symmer; ſed non ſufficientia. Ne vero longa narratione variorum tentaminum charta impleatur; palmaria tantum, et e quibus reliqua intelligi poſſunt, adferam. Nova ligamenta ſericea, unum circiter pollicem geometricum Suecanum lata, adhibui. Alterum extremitatibus fixum et bene tenſum, quod in poſterum *fricatum* voco, alterum vero ſeu *fricans* manibus, quantum ſatis, expando, ſuper eodem ſpatio fricati pluries duco reducoque perpendiculariter ad ejus longitudinem. Hanc operationem *tranſverſam* adpello: eam vero qua idem ſpatium fricantis ducitur antrorſum retrorſumque, ſuper tota longitudine fricati, *ſecundum longitudinem* fieri dico.

Si

Si jam fricans et fricatum fint ejufdem telæ, adeoque textura, color, fuperficies, ceteraque, quam proxime paria, fricatio vero inftituatur tranfverfa, fricans pofitivam et fricatum negativam contrahit electricitatem. In fricatione fecundum longitudinem fricans fit negativum et fricatum pofitivum.

Si fricans fit alius coloris quam fricatum (modo non nigri) eventus eft idem.

Si fricatum fuerit nigrum, hoc femper fit negativum, quocunque modo fricetur, excepto unico cafu, quo nimirum fricans quoque nigrum. Tunc enim, fecundum longitudinem fricatum, pofitive electrificatur.

Ex allatis vidi genus electricitatis folis circumftantiis frictionis determinari: ut vero modum invenirem, rem adcuratius penfitavi. In frictione tranfverfa, fricatum, magis quam fricans, teritur. Nominetur attritus fricati et fricantis F, f refpective, longitudo partis fricantis l, latitudo fricati a, numerus frictionum n, et erit femper $F = nf\ (l \div a)$. Hinc duplex oritur effectus; nam et fricatum magis lævigatur, et magis calefit, quam fricans. Politura alia corpora difponit feu aptiora facit ftatui pofitivo; ideoque, heic, negativum ei adfcribere nolui. Experimentum quoque feci, in quo fricans, multa frictione, erat lævigatum: fed nihilo minus, fricatum, rude, feu, nunquam antea adhibitum, inveniebatur pofitivum. Jure igitur in diverfum caloris gradum cecidit fufpicio: et hujus examinandæ ergo, varia tentavi; quibus tandem fequentia didici.

Si frictio inftituitur tranfverfa, fricans vero antea probe calefit, hoc invenitur, operatione peracta, negativum, et fricatum, pofitivum. Tentamina hoc fucceffu

ceſſu feci cum ligamentis, cœruleis, viridibus, luteis, rubris et albis.

Si fricatum fuerit nigrum, numquam poſitivam contraxit electricitatem, quamvis valde calefactum fricans: niſi hoc quoque nigrum.

Ex hiſce itaque merito concluditur: *Calorem corpora* (ſaltem nonnulla) *ſtatui negativo adaptare.*

Ubi de genere electricitatis quæſtio eſt, nonne hinc varii errores, dum alterutrum ex confricandis, antea, fortioris vis ergo, calefit? Nonne hinc phænomena cryſtalli Iſlandica, frigore producenda?

Nexum, experientiæ congruenter, ita concipio. Quodvis corpus B (e. g. vitrum raſum) fieri poteſt vel poſitivum vel negativum; prout, vel τῷ C (ſerico) vel τῷ A (panniculo laneo) adfricatur. Eſt igitur fixus quidam ordo, quem corpora ſemper ſequuntur, quamdiu circumſtantiæ manent eædem. Sint A, B, C, D, E, corpora; quorum quodlibet, cum antecedentibus, fit negativum, cum ſequentibus vero, poſitivum. Jam, quo minor inter confricata diſtantia, eo debilior, ceteris paribus, electricitas. Ideoque inter A et E fortior, quam inter A et B: ſi nimirum cetera paria. Hinc, inter corpora ejuſdem generis, difficulter ulla, producitur vis; quæ, e contrario, cum diſtantia, creſcit. Calor, uti probavi, ſtatui negativo corpora adaptat; ſed, ſi diſtantia memorata paullo major, eam quidem ſuperare non valet; quod ex ligamentis nigris patet; interim tamen vim debilitat. Si globus electrificatorius incaleſcit, experientia quotidiana edocti, novimus, vim electricam debilitari. Nonne inde, quod calore magis adaptetur ſtatui negativo, unde diſtantia fricans inter et fricatum minuitur? Sed de hiſce, alia vice, plura; dum ſimul caloris effectus in alia corpora exponere liceat.

De exiſtentia binarum electricitatum, moneo, multa variaque luculentiſſima experimenta plene convicerunt, ſed quid de natura electricitatis, ſic dictæ, negativa, ſit ſtatuendum, adhuc dubius hæreo. Apices poſitivi *flant*, et negativi quoque; quod ad oculum monſtrari poteſt; phoſphoro illito, nam vapores alias vage e corpore non electriſato ſurgentes, inſtar caudæ cometæ, tam poſitivi quam negativi, ad magnam diſtantiam ejectant. Difficulter cum privatione, rarefactione et adfluxu fluidi electrici conciliari poſſunt phænomena allata. Quæ tua ſit mens, ut mihi proxime explices, oro.

Quæ in noviſſima epiſtola de metallorum electricitate ſcripſi; de confricatione, iiſdem ſemper (quantum poſſibile) facta circumſtantiis, intelligenda ſunt: nam licet, initio, diu ſola frictionis variatione, diverſam, electricitatem procreare non poſſem, tandem vero, ad tuum præſcriptum, felicius operationem adgreſſus ſum.

Inſtrumentum novum nuper invenit dominus Wilcke, quo, in campo, abſque linea meridiana, acus magneticæ declinatio facile inveniri poteſt. Obſervationibus, hocce, Stockholmiæ factis, æſtate præterlapſa, acus 11 gr. 50 min. occidentem verſus, declinare reperiebatur. Hoc quoque utili invento, linea meridiana, abſque negotio, ducitur.

Domini Mountine et Dodſon, mappam, declinationes magneticas, pro anno 1756 (ſi probe meminerim) monſtraturam: polliciti ſunt, ſed, num talis in lucem prodierit, adhuc ignoro.

Epiſtola, ad R. Societatem Upſalenſem ſcripta, dudum lecta eſt.

Hocce autumno, varii globi ignei viſi ſunt, meteoro igneo, in parte prima voluminis LI. Tranſactionum

onum deſcripto, valde ſimiles. Fulmina quoque, æſtate præterita, in meridionalibus præcipue regni partibus, graſſata ſunt. Ullusne, in Anglia, fulminis ictus, virgis ferreis erectis, avertere conatus eſt? et quo ſucceſſu? In Penſylvania tentari mihi narratum eſt. Certe ſi prudenter inſtituatur, nulla hinc mala metuenda video.

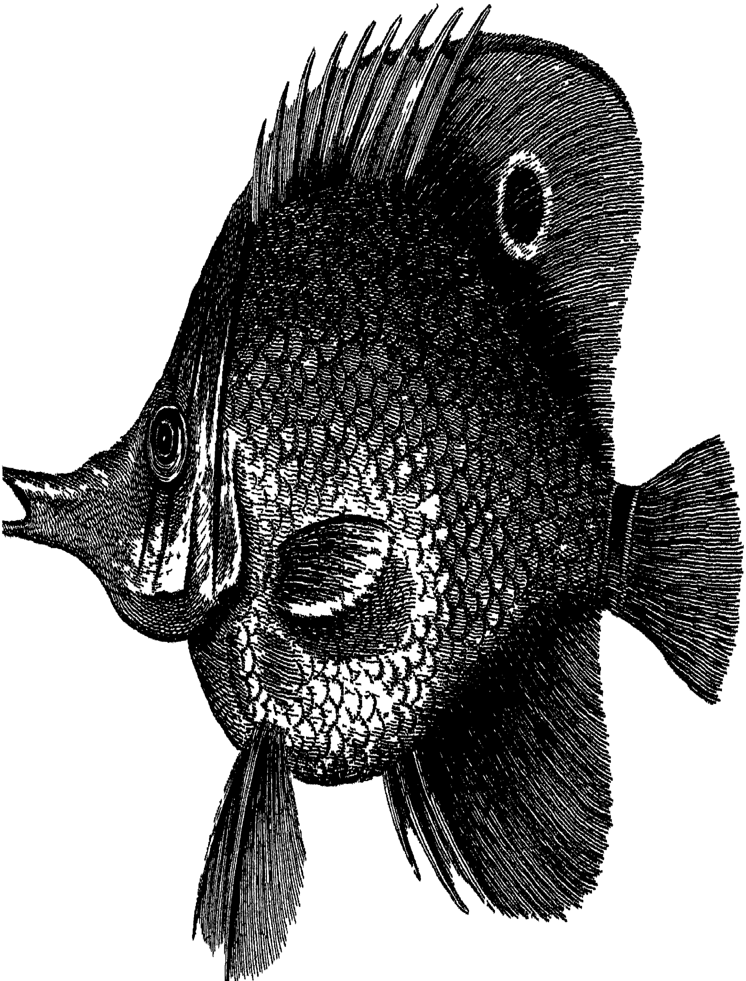
Omnigenam tibi optans felicitatem proſperitatemque, me tuo favori ſolitæque amicitiæ commendo, et ſummo, quo par eſt, animi adfectu, permaneo,

Ampliſſimi atque celeberrimi Nominis tui

Cultor obſervantiſſimus

Dabam Upſalæ,
d. 18 Oct. 1763.

Torbern Bergman.



The relation of this uncommon action of this cunning fiſh raiſed the governor's curioſity; though it came well atteſted, yet he was determined, if poſſible, to be convinced of the truth, by ocular demonſtration.

For that purpoſe, he ordered a large wide tun to be filled with ſea-water; then had ſome of theſe fiſh caught, and put into it, which was changed every other day. In a while, they ſeemed reconciled to their confinement; then he determined to try the experiment.

A ſlender ſtick, with a fly pinned on at it's end, was placed in ſuch a direction, on the ſide of the veſſel, as the fiſh could ſtrike it.

It was with inexpreſſible delight, that he daily ſaw theſe fiſh exerciſing their ſkill in ſhooting at the fly, with an amazing velocity, and never miſſed the mark.

In looking over that noble work of the Muſeum of the king of Sweden, printed anno 1754, I met with this Jaculator, well engraven, and deſcribed, by the learned Baron Linnæus, under the title of Chætodon, pag. 61, plate 33.

Baron Linnæus's Deſcription.

Acanthopterygii-chætodon.

Chætodon roſtratum, pinna dorſali poſtice macula fuſca.

Corpus ovatum, compreſſum; faſcia griſea perpendicularis ſecat caput per oculos. Faſcia griſea perpendicularis ab initio pinnæ dorſalis deſcendit ante pectorales ad ventrales. Faſcia griſea perpendicularis in

medio piſce. Faſcia fuſca ſaturatior cingit caudam ante radios. Macula fuſca orbicularis in medio pinnæ dorſalis ubi mutica.

Caput roſtro elongato, fere ut in ſyngnathis, dentes in maxillis minimi. Narium foramina utrinque 2 ante oculos, membranæ branchioſtegæ oſſicula 5. Opercula branchiarum ſquamis tecta, ut in reliquis congeneribus. Pinnæ dorſi et ani æquales, valde tranſverſæ, et lateribus ſquamis tectæ. Dorſalis radiis 9–31 primoribus mucronatis, poſterioribus 31 mollibus, longioribus. Pectorales radiis 14. Ventrales radiis 6 mollibus, excepto primo ſpinoſo; eorum ſecundus reliquis longior. Ani radiis 3–20 poſterioribus 20 longioribus, mollibus; primis 3 ſpinoſis, caudæ radiis 14, æqualibus; parva

Accedit proxime ad LABRUM roſtro reflexo faſciis lateralibus tribus fuſcis. Amœn. Acad. I. p. 313.

XIV. *An Account of the Poliſh Cochineal: In a Letter to Mr.* Henry Baker, *F.R.S. from Dr.* Wolfe, *of* Warſaw.

Warſaw, April 4, 1763.

Read March 29, 1764.

COCCI Polonici ſunt ova, vel potius pupæ inſecti nondum ſatis cogniti, quæ ad radices variarum plantarum adhærent, et verſus finem Julii ab evulſis radicibus ope cultri abraduntur et colliguntur. Plantæ illæ ſunt valde variæ, nec quotannis in una eademque ſpecie reperiuntur

tur cocci illi, ſed pro lubitu vagantur, hoc anno in hac, ſequenti in alia planta. Communiter creditum, non inveniri niſi in ſclerantho perenni calycibus fructus clauſis, Linnæi, quod polygonum minus Bauh. folio et flore albicante, ſeminibus nudis oblongis. Hæc planta amat loca ſabuloſa; ſed nimis eſt rara, ut notabilis cocci quantitas inde colligi poſſit. Uberior longe proventus eſt ejus in pratis pinguibus Podoliæ et Ucrainiae: ibique invenitur ſupra omne genus fragariæ et potentillæ; ſæpe etiam ad radices ſecalis, aliarumque plantarum, de quibus tamen nihil certi compertum habeo. Maxima copia collectum vidi ex potentilla alba Linnæi, fol. digitat. 5 natis, apice conniventi ſerratis, caulibus filiformibus procumbentibus, receptaculis hirſutis: hanc nimirum indicare mihi videtur; ceterum ex fragraria flore albo, foliis lanceolatis, medio maximo, ſubtus villoſis, ſupra viridibus cum tenui margine argenteo, caulibus debilibus hirſutis. Deinde ex pentaphyllo officinali, ſeu potentilla reptante Linnæi, fol. quinatis, caule repente, pedunculis unifloris. Poſtea etiam ex potentilla caulefcente Linnæi fol. quinatis apice conniventi ſerratis, caulibus multifloris erectis receptaculis hirſutis; de quibus ſpecimina mitto.

Poſtquam copia horum cocculorum collecta eſt, immittuntur in ollam, et ſupra ignem torrentur quouſque vermes enecati arbitrantur. In Auguſto, inſectum, ovum ſuum relinquit, et in planta tarde decurrit. Eſt inſectum ſeminis cannubis magnitudine, totum molle, infra planum, ſupra ellipticum, ſeu ovatum, rugis tranſverſis ſemicircularibus decem circiter a capite ad anum, quæ rugæ in inferiori abdominis parte in marginem quaſi vel fimbriam coëunt, ſecundum circumferentiam abdominis inferiorem.

Caput

Caput parvulum; thorax ſupra vix conſpicuus. Color totius animalis obſcure purpureo-brunnus. Totum corpus pilis tenuibus, argenteis longis (reſpectu inſecti) undique tomentoſum, ut videatur pulverulentum, vel farina alba conſperſum. Pedes ſex valde breves, minuti, nigrore ſplendentes, inſtructi unguibus acutis duobus. Antennæ duæ filiformes perbreves nigerrimæ: roſtrum reflexum perbreve. An abdomen pone ſetoſum? ut dicit Linnæus. Saltem pili ibi videntur paulo craſſiores et longiores, ſed ſimiles reliquis. An volatilia fiant, expiſcare nondum potui, nec ſexum quidem internoſcere potui. Sed dabitur, Deo dante, opportunior occaſio, in hæc inquirendi. Transformationes difficulter obſervantur, cum inſectum delicatulum a quavis injuria facile vitâ privetur, et illo tempore intra fiſſuras radicum abſcondat ſe. Optimam figuram hujus inſecti nuper dedit Ledermüller Norimbergenſis in obſervationibus microſcopicis.

Color inde lanæ, goſſipio, lino, conciliatur dilute carmeſinus. Modus tingendi talis eſt. Coquunt coccum in aheno cupreo, cum liquore, quem *kwas* (acidum) dicunt, et qui in Podolia, Ruſſia, et Ucrainia, pauperibus pro potu ordinario inſervit. Parant vero hunc potum *kwas* ex farina ſecalina, quam infundunt aqua multa calida, et in loco tepido relinquunt, donec fermentatione aceſcat, et limpida fiat. Quantum quotidie de hoc liquore ebibunt, tantum addunt novæ aquæ cum manipulo farinæ. Breviori tempore idem fit, ſi fermentum acidum panis ſecalini piſtorum cum multa aqua diluatur, et in locum tepidum reponatur. Jam in hoc liquore coccum diu coquunt: Enaſcitur ſpuma et pinguedo valde multa, inſtar ſebi alba, quam ſollicite ſemper auferunt, uſque dum talis jam nihil appareat. Erit liquor pulcre ſangui-

neus.

neus. Jam, lanam puram albam in alio aheno cum ſimili liquore *kwas*, et mediocri aluminis quantitate decoquunt, et ſalibus his bene imbutam exſiccant. Tandem lanam ita præparatam, in liquorem illum ſanguineum immittunt, et per aliquot minuta coquunt: ſic in momento omnis color lanæ adhæret, et liquorem inſtar aquæ limpidum relinquit. Lanam ſic tinctam aqua frigida abluunt et exſiccant.

Rudis hæc tractatio docet, quantum ille color emendari poſſet, ſi in vaſe ſtanneo, cum ſale ammoniaco et ſolutione ſtanni tractaretur. Narrarunt mihi collectores, ſi animalcula viva colligantur et enecentur, colorem inde obtineri multo elegantiorem; cui facile crediderim, ſi præſertim eadem ſollicitudine colligerentur, ac fit cum cocco Mexicano (cui de cetero noſtrum inſectum valde ſimile videtur), et loco toſtionis, in aceto enecarentur. Multum Chocimi inquiſivi in id, quo Turci purpureo colore lanam inficiunt: ſed tinctura illa non niſi in Aſia minori exercetur. Omnes tamen dicunt, tincturam hanc obtineri ex baccis, quæ ad radicem plantæ Armeniacæ, quam Romam appellunt, creſcunt. Forte hæc planta eadem cum potentilla alba, et forte etiam pulchritudo coloris non niſi ab artificio tinctoris pendet.

Quantitas cocci hujus ad exteros exportati, ex Podolia, facile aliquot millia librarum quotannis excedit, et præterea multum domi conſumitur. Maxima pars in Turciam abvehitur, magna etiam Breſlaviam venit. Conſtat libra una 8–10 florenis Polonicis, (ſive 4–5 ſhillings) et unâ librâ fere 20 libræ lanæ tingi poſſunt.

Further

Further Account of the Polish *Cochineal: In a Letter from Dr.* Wolfe *to Mr.* Henry Baker, *F. R. S.*

Read April 12, 1764.

LAST summer I amused myself with the Polish cochineal. It is unknown and neglected in this country. The several kinds of Potentillas are here very rare, and it was only upon the polygonum minus, or scleranthus perennis Linnæi, that I found the cochinille. I gathered about 300 of the coccusses, and put them with the plants and some sand in large pots. They are of different sizes. The insects creep out of their coccusses from the beginning of June till the middle of August: about fifty got out under my eyes. They are all exactly of the same shape: but some are three times smaller than others, according to their coccusses. The coccus is a thin round white skin. The insects are all hairy more or less; some are of a darker colour, some more crimson; some seem somewhat longer, others broader. But these differences seem to depend on their voluntary extension, and on their age, because they grow from day to day darker and more hairy. No mouth is to be seen, but a deep plait or furrow at the upper part of the breast. Two extremely small dark points seem to be the eyes. The two horns are thick, twisted like a screw, of the length of the breast; they end in an obtuse point. The two fore legs are twice the size of the four hinder legs, they have all sharp black incurved claws. The shape of the wrinkles and furrows may be seen in the drawing. It is impossible to find marks of the sex; and though they join sometimes their anusses, yet they do it so loosely,

loofely, that it cannot be accounted for a copulation. They feem to eat nothing at all. They creep about the plant a week or two, going often under ground, and getting up again. Then they make themfelves a deep cylindrical hole in the fand down to the hard bottom of the pot, the end of which they cover with a fine white filk growing upon their bodies. There they lay their eggs and die. Others, who are difturbed in their work, grow weary and white, as if they were powdered all over with a white meal, which through a glafs appears to be very fine white filky hairs, coming out over all the body. At laft they lay them down upon their backs: the filky hairs grow very faft, to the length of one inch and a half, and the infect twifts with its claws the hairs all round its body, fo as to refemble a fmall heap of cotton; but the hairs are fo tender, that a fmall wind will tear and deftroy it. In this heap of cotton they lay their eggs, from fifty to an hundred, and then they die. Thus they remain till the middle of July. Afterwards, though they make their holes, or their cotton heaps, yet they die without laying eggs. The eggs are crimfon, tranfparent, fcarce vifible, long, and round-pointed at both ends. In a week's time the young infects creep out: they are like their parents, but fmooth, tranfparent, and crimfon. I prefented them every day frefh roots of the polygonum, but I cannot fay they have eat any of them. In a week or two they difappear, going under ground. I preferve all thefe things. The infects feem now all dead, and fo do the young ones, buried up in fand: but I hope next fpring to fee them alive, and to profecute their farther change. I have killed about one hundred of the

the insects in hot vinegar, as it is done in Mexico; and now I shall attempt to dye some woolly threads in the common way of the scarlet dyers. In the microscopical observations of Ledermuller at Nuremberg, you will find tolerable drawings belonging to this matter. In the beginning of August, I found an extremely small white fly, somewhat like to what is supposed to be the male insect. It is a third part of the size of what is represented by Ledermuller. It has a body like a gnat, snow-white, powdered below, but gray shining upon its back, six tender snow-white legs without claws, a thick bulky head, two very small prominent eyes, two hair-like horns, two wings, large enough in comparison to the body, snow-white below, and shining gray above. The belly to the tail is taper, and at the tail are three white hairs, very tender, and four or five times the length of the whole fly. But as this was the single one amongst three hundred, and totally unlike in every part to the other insects, I doubt very much of its being of this genus.

I hope next summer will teach me more; and, if I should be happy enough to bring the matter to any clearness, I shall put my observations into some order, and send them over with proper drawings belonging to it. But as there is no doubt but this insect will be found as well in England as in Poland, I thought it proper to give you the account of my observations as far as they go. Perhaps somebody of yours will think it worth their while to look the next month of June at the potentilla, fragaria, and polygonum minus roots, and will very likely find these same things.

Tab. X. N°. 1. The cochineal insect of its natural size. 2. The same magnified. 3. The cotton. 4.

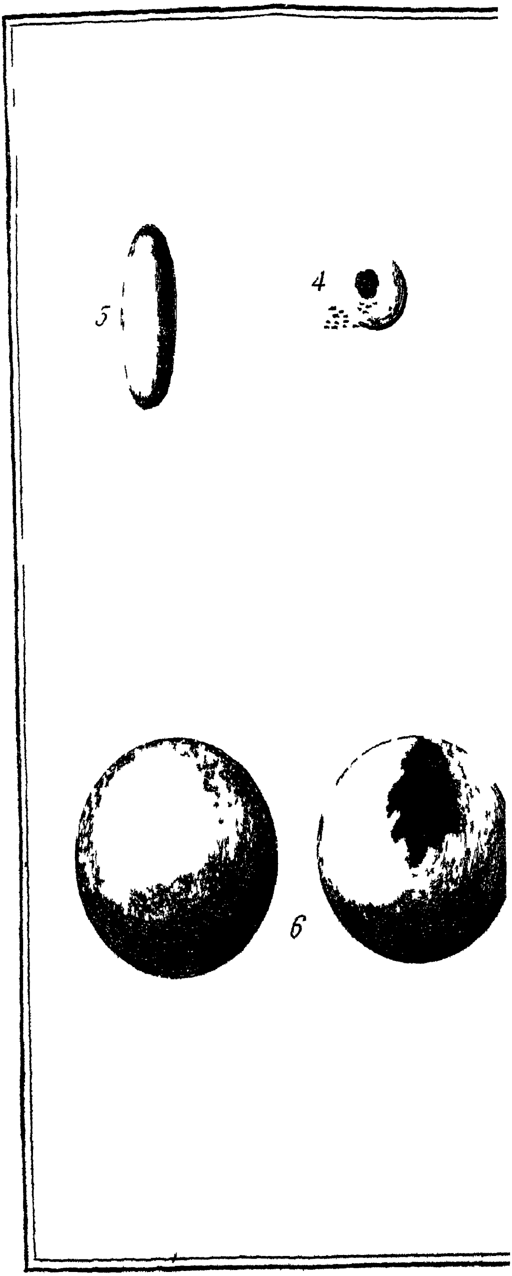
 The

The cotton with the animal in the middle, and its eggs of the natural ſize. 5. An egg by the firſt magnifier. 6. Two coccuſes greatly magnified. 7. The inſect greatly magnified.

I ſend you alſo ſome of the inſects killed in vinegar and dryed. The cotton, and the ſuppoſed male inſect. Some young inſects. Some dead inſects buried up in their cotton, ſome of which layed eggs, others not, ſome void coccus ſhells, ſome young ones, ſome eggs, etc. and alſo the polygonum minus.

P. S. The 12th of October, at 8 o'clock in the evening, we had here a ſtrong aurora borealis. It laſted but a quarter of an hour. The ſhooting rays were white, and went all round from the horizon, making up at leaſt three quarters of the circle of the horizon, the middle being juſt in the north. The rays pointed all towards one point of the heaven, which point was not the zenith, but at leaſt 20 degrees farther directly againſt the South. It was a fair day. No wind or rain followed it; but the air was calm before and after.

Warſaw, Nov. 23, 1763.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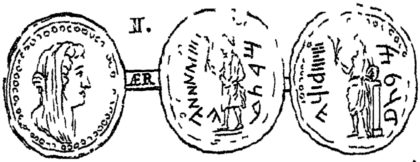
J. Mynde sc.

PUNIC *and* PHŒNICIAN *Coins*.

1.

Apud Joannem Swinton, S.T.B Oxonienf. R.S.S.

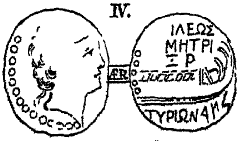
II.



III.

Penes Joannem Swinton, S.T.B. Oxonienf. R.S.S.

IV.



Apud Joannem Swinton, S.T.B. Oxonienf. R S.S.

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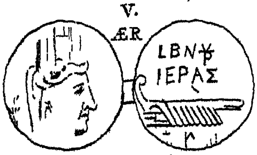
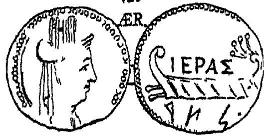


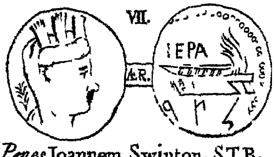
In Scrin Wakian.

VI.



Penes Joannem Swinton, S.T.B. Oxonienf. R.S.S.

VII.

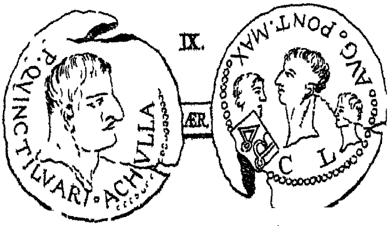


Penes Joannem Swinton, S.T.B. Oxonienf. R.S.S.

VIII.

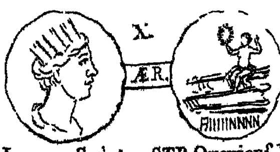
Apud Joannem Swinton, S.T.B. Oxonienf. R.S.S

IX.



Ex Sched. Lethieullierian.

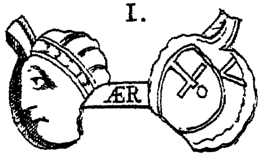
X.



Penes Joannem Swinton, S.T.B. Oxonienf. R.S.S

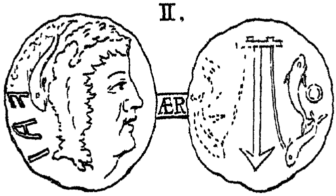
J. Mynde

I.



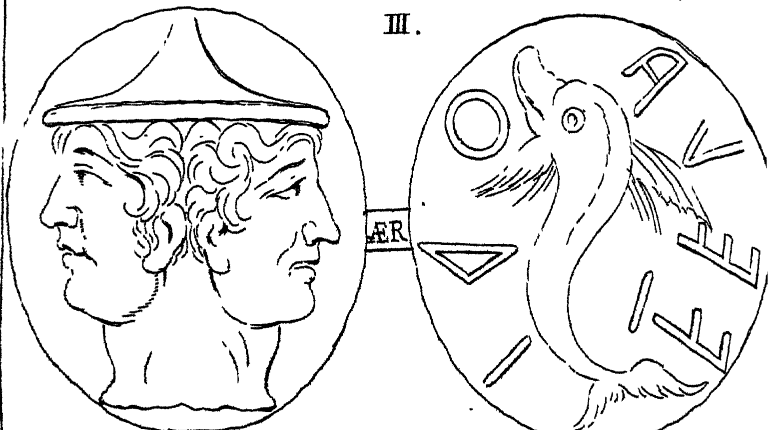
Penes Joannem Swinton, S.T.B. Oxonienſ. R.S.S.

II.



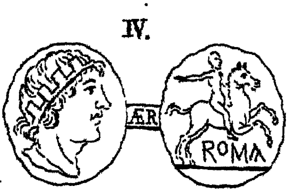
Penes Joannem Swinton, S.T.B. Oxonienſ. R.S.S

III.



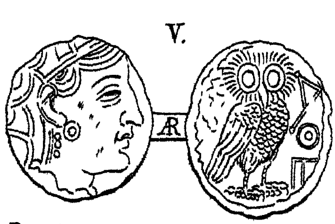
Apud Joannem Swinton, S.T.B. Oxonienſ. R.S.S.

IV.



Apud Joannem Swinton, S.T.B. Oxonienſ. R.S.S.

V.



Penes Joannem Swinton, S.T.B. Oxonienſ. R.S.S.

J. Mynde ſc.

XVI. *Obſervations upon two antient* Etruſcan *Coins, never before illuſtrated or explained. In a Letter to the Rev.* Thomas Birch, *D. D. Secret. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany.

Good Sir,

Read March 29, 1764.

TWO ſmall Etruſcan coins, or weights, came lately into my poſſeſſion, which I take to have been never hitherto publiſhed. Each of them is an uncia, or twelfth part of an as, and in pretty good conſervation. The weight, or value, of each piece appears from a ſingle globule on the reverſe. As on ſeveral accounts they ſeem to merit the attention of the curious, and have not been yet explained; the Society will indulge me the liberty of imparting to them my ſentiments of theſe valuable coins, and ſubmitting to their conſideration a few curſory remarks upon them here. [Tab. XI.]

I.

The firſt of theſe medals preſents to our view a diademated head, greatly deformed by the injuries of time.

time. The workmanſhip is rude, ſuch as we find it to be in many of the more antient Etruſcan coins. The ſlip of metal projecting from the round of the weight demonſtrates the piece to have been caſt, and may therefore be conſidered as a certain indication of it's high antiquity. From the globule and two letters, T V, on the reverſe, we may infer this coin to have been a ſtips uncialis of the Tudertes, or people of Tuder, ΤΟΥΔΕΡ, as this antient city of Italy ſeems to have been called by (1) Strabo. It is at preſent known by the name of Todi. It went under the denomination of Tutere (2) amongſt the Etruſcans, and is pointed out to us by the letters T V, as here, on (3) ſeveral of their earlier coins.

That this piece is an Etruſcan weight of very conſiderable antiquity, ſeems likewiſe evident from the barbarous and uncouth taſte that appears in every part of it, in conjunction with the manner of writing, from the right hand to the left, viſible on it, which antiently prevailed over all the (4) lucumonies of Etruria. The figure on the reverſe, however rude and inelegant it may be, was perhaps intended to repreſent the prow of a ſhip, ſo frequently to be met with on this minute ſort of coins. For a particular and ſatisfactory account of the early origin of

(1) Strab. *Geograph.* Lib. x. Sil. Ital.

(2) Anton. Franciſc. Gor. *Muſ. Etruſc.* p. 162, 427. Florentiæ, 1737.

(3) Anton. Franciſc. Gor. ubi ſup. p. 427. Honor. Arigon. *Num. Urb. et Populor. Hetrur.* Tab. VII, VIII, VIIII, &c. Tarviſii, 1745.

(4) Phil. Bonarot. *Ad Monument. Etruſc. Oper. Dempſt. Addit. Explicat. & Conject.* § xlii. p. 90. Florentiæ, 1726.

ſuch

ſuch caſt pieces (5) as this, recourſe may be had to the very learned authors referred to here.

With regard to the diademated head on this piece, I ſhall only beg leave to obſerve, that a ſimilar one occurs on an antient Etruſcan coin publiſhed by (6) Arigoni, on a very old medal of Rome now in my poſſeſſion (7), and undoubtedly on ſeveral other valuable pieces of a very remote antiquity, to be met with in the cabinets of the learned. This ſmall Etruſcan uncia weighs preciſely three penny weights and one grain.

That the weight, or coin, I have been conſidering was current in Etruria long before the people of that country found themſelves obliged to ſubmit to the Roman yoke, will not, I think, admit of a diſpute; but how many years it preceded that wherein the battle of the Lacus Vadimonis (8), ſo ruinous to the Etruſcans, was fought, for want of ſufficient light, both from hiſtory and the medal itſelf, I muſt not take upon me to decide.

II.

The ſecond piece exhibits on one ſide the head of Hercules, adorned with a lion's ſkin; behind which a fiſh, reſembling the turſo, or tyrſo, appears, attended by three Etruſcan letters well enough preſerved. On the reverſe a dolphin, or tyrſo, part of an anchor, and another fiſh, under the former, preſent themſelves

(5) Idem ibid. § xxxviii. p. 78. Anton. Franciſc. Gor. ubi ſup. p. 421. *Una Lettera Al Signor Abate Barthelemy di* Annibale degli Abati Olivieri, p. 27. In Peſaro, 1757.

(6) Honor. Arigon. ubi ſup. Tab. XIV, XVI.

(7) See Plate Fig.

(8) T. Liv. *Hiſtoriar. ab Urbe Cond.* Lib. ix.

to

to our view. The other part of the anchor and the fish appertaining to it have been defaced, by the injuries of time; as we may collect from a similar stips uncialis formerly published by F. Montfaucon, then in the cabinet of the Marshal d'Estrées. A single globule, or uncial mark, determining the weight, or value, of the piece, is also visible here. The workmanship is somewhat rude, and different from that of the Romans. The tyrso seems to allude to the origin and most antient name of the Etruscans, who were called Tyrsenians by the Greek writers that flourished before Polybius (10). That fish not seldom (11) occurs on the Etruscan coins. It is thought to have been one of the distinguishing sym-

(9) Montfauc. *Supplem. de l'Antiquit. Expliqu.* Tom. III. Liv. iv. c. 6. Pl. 48. p. 106. A Paris, 1724.

(10) Boch. *Chan.* Lib. I. c. XXXIII. p. 647, 648. Francofurti ad Moenum, 1681. Phil. Bonarot. *Ad Monument. Etrusc. Explicat. & Conject.* § XXXVIII. p. 80. Florentiæ, 1726. Anton. Francisc. Gor. *Mus. Etrusc.* Vol. II. p. 421. Florentiæ, 1737.

(11) Phil. Bonarot. ubi sup. § xxxviii. Ant. Fran. Gor. ubi sup. p. 421. I have an exceeding fine Etruscan weight, or brass medal, of Volterra, adorned with the head of Janus on one side, done after the Etruscan manner; and on the reverse with a tyrso, the obelus, or mark of the as, and seven Etruscan letters, forming the word VELITHERA, or VELITERA, the old Etruscan name of Volterra, one of the most antient cities of Tuscany. The piece is in the finest conservation, and weighs four ounces, fifteen penny weights, and eighteen grains.

It is farther remarkable for a figure of the A, which I never met with, before this weight fell into my hands, upon any of the Etruscan remains of antiquity. One of the sides of that element here is much shorter than the other, not unlike the form of the Greek *Alpha* exhibited by some of the earliest Athenian tetradrachms, particularly one in my possession, the most antient of any that I have hitherto seen.

bols

bols of the Tyrſenian nation, with which the people of it were very well pleaſed.

The forms of the letters on this medal are exactly the ſame with thoſe of the correſpondent elements uſed in Umbria and the Proper Etruria, and apparently anſwer to the Roman letters FAI. They are ſufficiently clear and diſtinct, having ſuffered little from the injuries of time. The word they repreſent, in conformity to the Etruſcan (12) manner, points out to us ſome remarkable town. This may be certainly evinced from the inſcriptions preſerved on other Etruſcan coins. Now to what town can this word be ſuppoſed as a proper name to refer, but to Fæſulæ in the Proper Etruria? That city ſtood at the foot of the Apennines, and was one of the moſt (13) antient, as well as the moſt conſiderable, towns of Etruria. The Greeks wrote the name of it (14) ΦΑΙΣΟΥΛΑΙ and (15) ΦΑΙΣΟΛΑ, the earlier Latins (16) FAISVLAI,

Hence it ſhould ſeem to appear, that this valuable Etruſcan coin has never yet been publiſhed, at leaſt with a ſufficient degree of exactneſs. I therefore judged it would be by no means improper to ſend the Society a draught of it, as well as one of the Athenian tetradrachm here mentioned, taken with the utmoſt accuracy, and ſuch as may be entirely depended upon.

Vid. Annib. degl. Abat. Olivier. ubi ſup. p. 43.

(12) Anton. Franciſc. Gor. ubi ſup. p. 422–431. Joan. Baptiſt. Paſſer. Piſaurenſ. *De Num. Etruſc. Pæſtanor. Diſſertat.* Vid. *Symbol. Litterar.* Vol. II. p. 13–35. Florentiæ, 1748.

(13) Tho. Dempſt. *De Etrur. Regal.* Lib. IV. c. XIX. & alibi, Florentiæ, 1724.

(14) Appian. Alexandrin. *De Bell. Civil.* Lib. II. p. 711. Amſtel. 1670. Dio, Lib. XXXVII.

(15) Polyb. *Hiſtor.* Lib. II. c. 25. p. 158. Amſtel. 1670.

(16) Enn. & Lucret. paſſim. Virg. Joh. Nicol. Func. Marburgenſ. *De Adoleſcent. Latin. Ling. Tractat.* p. 325. Marburgi Cattorum, 1723.

and those of the Augustan age FAESVLAE. The diphthong AI at first prevailed amongst the Latins, as well as the Greeks; but was afterwards converted by them into Æ, or AE.

That the Etruscans used globules on their coins, to denote the weight or value of them, has been observed by the most celebrated (17) antiquaries, and is now universally allowed. From them the custom of impressing these marks upon money and weights passed to the Romans. Now Fæsulæ was one of the most famous and antient cities of Etruria. Nay, with its district, according to a (18) very eminent writer, it formed one of the twelve lucumonies, or free states, of that country; and, in support of this sentiment, it may be remarked, that several valuable remains of (19) Etruscan antiquity have been found near the spot formerly occupied by that city. A mint was therefore undoubtedly erected there, and money coined in it. The situation of this place, at a small distance from the Arno, and not far from the Tyrrhenian Sea, may be considered as an additional reason why a turso, or tyrso, should have sometimes been impressed on its coins.

The age of the piece before me I cannot take upon me, with any precision, to determine; but think it must have been emitted from the mint at Fæsulæ, before the final subjugation of Etruria by the Romans.

(17) Phil. Bonarot. ubi sup. Honor. Arigon. *Num. Urb. & Populor. Hetrur. Antiquissim.* Tarvisii, 1745.

(18) Dempst. ubi sup. et alibi. Vid. etiam Cluver. Cellar. &c.

(19) Anton. Francisc. Gor. ubi sup. p. 19, 112, 435.

We may therefore reasonably presume it to have appeared before the reduction of that country to the form of a province by the consul Ti. Coruncanius, in the year of Rome 473; if not before the terrible overthrow given the Etruscans by the consul Æmilius Barbula, in the year 442, which seems to have put a period to the independency of that nation. If we admit this, the coin cannot well be supposed to have preceded the birth of CHRIST less than three hundred years. Nay, it may be of a much earlier date, if the learned (20) Sig. Gori's notion of the high antiquity of the brass Etruscan coins be not altogether remote from truth. Be this as it will, no one has yet fully disproved or invalidated his opinion.

From the coin here considered, I think, we may venture to infer, that the manner of adorning with the head of Hercules some of the lesser weights was (21) originally Etruscan, but adopted afterwards by the Romans. The people of Fæsulæ and the neighbouring tract undoubtedly formed one of the twelve lucumonies, or cantons, of Etruria, and even made a figure after the Etruscan times. A colony was settled here by (22) Sylla, and the inhabitants of this place seem to have enjoyed the priviledges of a municipium in the days of (23) Pliny. Other points, besides those already mentioned, are clearly deducible from the medal I

(20) Idem ibid. p. 419. Phil. Bonarot. ubi sup.

(21) Anton. Francisc. Gor. ubi sup. p. 424, 425. Phil. Bonarot. ubi sup.

(22) Cic. in *Catilin.* Orat. III.

(23) Plin. *Nat. Hist.* Lib. VII. c. XIII. p. 381. Ed. Hard. Parisiis, 1723.

have been endeavouring to explain, the only one of Fæſulæ hitherto diſcovered, which at preſent I cannot ſo much as touch upon; having but juſt time to aſſure you that I am, with the moſt perfect conſideration and eſteem,

S I R,

Your moſt obedient humble ſervant,

Chriſt-Church, Oxon,
May 31ſt, 1763.

John Swinton.

XVII. *Obſer-*

XVII. *Observation of the Eclipse of the* Sun, *the 1st of* April 1764, *made in* Surry-street, *in the* Strand, London: *By* James Short, *M. A. F. R. S.*

Read April 5, 1763.

THE morning of the eclipse I had provided the instruments I judged would be necessary for observing it in such a manner as to be satisfactory to the Royal Society as well as to myself. A reflecting telescope of two feet focal length, it's aperture four inches and a half, and it's magnifying power seventy times. To this telescope was fitted a micrometer with an achromic object-glass of 40 feet focal length.

The right honourable the Earl of Morton, now President of this Society, was pleased to honour me with his company, and also to observe; but in different rooms, out of sight and hearing of one another. His Lordship used a reflector of only eighteen inches focal length, four inches and a half aperture, and a power of forty times, to the eye-piece of which a helioscope was adapted, for viewing the Sun distinctly, without the least inconvenience to the eye.

The condition of the air was very unpromising, for, besides a general haziness of the sky, thin slow moving clouds were frequently passing over the Sun from

from the South-weſt, ſo that it was by fitts only that the Sun's limb could be ſeen diſtinctly. I uſed a ſmoaked-glaſs to defend my eye, and my obſervations were noted down as follows.

	Apparent time. h	′	″	
March 31,	21	4	33	the beginning of the eclipſe by me.
		4	36	by Lord Morton.

All the reſt by myſelf, with the before mentioned micrometer.

h	′	″		′	″	
21	12	27	—	13	22, 0	= diſtance of the cuſps.
	14	12	—	14	32, 1	= ditto.
	16	17	—	15	50, 8	= ditto.
	18	1	—	16	50, 6	= ditto.
	19	37	—	17	45, 4	= ditto.
	48	42	—	27	7, 1	= ditto.
22	19	15	—	29	33, 2	= ditto.
	22	28	—	29	49, 5	= Moon's diameter nearly parallel to the horizon.
	23	58	—	29	49, 5	= ditto.
	26	10	—	2	58, 7	= greateſt diſtance of Sun and Moon's limbs.
	28	28	—	2	31, 3	= ditto.
	30	43	—	2	26, 2	= ditto.
	32	8	—	29	49, 5	= Moon's diameter nearly parallel to the horizon.
23	35	23	—	21	11, 4	= diſtance of the cuſps.
	37	33	—	20	18, 4	= ditto.
	40	59	—	18	52, 9	= ditto.

The

The end could not be seen for clouds, but the whole of the eclipse may be determined from the above measurements.

The Sun's diameter parallel to the horizon, about an hour before noon on the day of the eclipse, was 31′ 59″, 4, air hazy. The next day at the same hour it was 31′ 58″, 6.

ADDITION.

	h	′	″				′	″	
April 12.	11	0	0	P. M.	Moon's diameter measured	=	32	49, 0	
13.	6	30	0	P. M.	——— ———	=	33	8, 8	
	10	25	0	P. M.	——— ———	=	33	20, 7	air undulating.
14.	6	30	0	P. M.	——— ———	=	33	21, 6	
	11	0	0	P. M.	——— ———	=	33	39, 5	air undulating.

XVIII. *Observation of the Eclipse of the* Sun, April 1, 1764: *In a Letter from Dr*. John Bevis, *to* Joseph Salvador, *Esq*; *F. R. S.*

SIR,

Read April 5, 1764.

THE honour you were pleased to do me by sending me an invitation to observe the late eclipse of the Sun at your house, and the accommodations I there met with, require that I should give you the best account I can of my observation, however imperfect through the unfavourableness of the we

You may remember, Sir, that, not long before the beginning of the eclipſe, the air, from very ſerene, turned hazy, and thin clouds came from the South-weſt. I had ſet a ſtop watch of Graham's, by a clock likewiſe of his, with which the Sun's tranſit on the meridian was obſerved carefully two days before the day of the eclipſe, and the next day after it. By comparing my watch with this clock on my return, I found it was 19 ſeconds too faſt in mean time, at your houſe, when I obſerved the beginning; and whereas it then ſhew'd $9^h\ 9'\ 12''$, it ſhould have ſhewed no more than $9^h\ 8'\ 53''$, from whence ſubducting $3'\ 45''$, the equation of time, there remains $9^h\ 5'\ 8''$, the apparent time of the beginning of the eclipſe, as I obſerved it.

But I muſt remark, that, the ſtate of the ſky continuing ſuch as I have deſcribed it, the beginning muſt have really happened ſooner, by 10 or 15 ſeconds, as I judge from the firſt perceivable diſtance of the cuſps; ſo that, if I ſtate it at $9^h\ 4'\ 53''$, I preſume I ſhall err but a very few ſeconds.

About the middle of the eclipſe, the air was very clear, and the cuſps well defined, which wanted about 60 degrees of joining. I could not then diſcern any thing on the Sun about the Moon's limb, which in the leaſt indicated a lunar atmoſphere. A full digit of the Sun, or more, remained uneclipſed. The day-light was but inconſiderably diminiſhed, ſo that neither Jupiter nor Venus could be ſeen, though both in a favourable poſition, to the eaſt of the Sun.

Fahrenheit's thermometer, placed without door to the north, ſtood at 50 when the eclipſe began, and fell but one diviſion whilſt it laſted.

The

The end of the eclipſe could not be obſerved for thick clouds, to the great diſappointment of all who had the curioſity to give their attention to this phænomenon in or about London.

I am, with great regard,

SIR,

Your very obedient humble ſervant,

Clerkenwell-Cloſe,
April 4, 1764.

J. Bevis.

The Moon's *Eclipſe of* March 17*th*, 1764, *obſerved in* Surrey-ſtreet, *in the* Strand, London.

Apparent time. h	′	″	
10	32	0	the penumbra juſt ſenſible to the naked eye.
	39	0	the beginning, viewed with an opera glaſs.
	48	30	the ſhadow touches *Mare Humorum*: This, and thoſe that follow, with a 9 inch reflector.
	53	35	*Tycho* touched by the ſhadow.
	54	41	*Grimaldi* touched.
	57	30	*Grimaldi* covered.
11	46	30	the ſhadow touches the ſouthern border of *Mare Tranquillitatis*.
0	49	36	——— touches the ſouthern border of *Mare Criſium*.
12	13	44	*Grimaldi* begins to emerge.
	17	36	——— is quite emerged.
	56	23	*Mare Criſium* nearly biſected by the ſhadow.
13	0	30	*Tycho* out of the ſhadow.
	2	40	*Mare Criſium* out of the ſhadow.
	16	30	the end of the eclipſe, with an opera glaſs.
	24	0	the Moon clear of the penumbra.

The ſhadow was ill defined, tho' the air was clear.

P. S. I find an obſervation of mine of the lunar eclipſe of May 7th, 1762, printed in the Philoſophical Tranſactions Vol. LII. pag. 543. How it got there I know not, never having thought it worthy the notice of the Royal Society. Beſides, in the three obſervations there ſaid to be made at Mr. Short's before the eclipſe, the equation (I ſuppoſe by my own miſtake) is *ſubſtracted*, which ſhould have been *added* to give the apparent time; and the three apparent times there ſet down, are to be increaſed each by 4′ 16″ to make them the true ones. J. B.

XIX. *Obſervations on the Eclipſe of the* Sun, April 1, 1764: *In a Letter to the Rev.* Thomas Birch, *D. D. Secret. R. S. from Mr.* James Ferguſon, *F. R. S.*

Reverend Sir, Liverpool, April 2, 1764.

Read April 5, 1764.

HAVING been at this place ever ſince the beginning of March, and hoping that the ſky would prove favourable (as to my great joy it did) for obſerving both the lunar eclipſe of March 17th, and the ſolar eclipſe of yeſterday, I propoſed to captain Hutchinſon, at whoſe houſe I ſtay, to have a meridian line drawn on the leads on the top of his houſe, in order to adjuſt his clock for obſerving the times of theſe eclipſes by: and we got Mr. Holden, who is maſter of a mathematical ſchool here, to do it for us, by ſeveral obſervations of the

altitude

altitude and azimuth of the Sun by day, and of the ſtars by night; and there were ſuch exact agreements found by many repeated obſervations, that no doubt could remain of the meridian's being very well aſcertained. The ſame gentleman, who is juſtly eſteemed to be a very accurate obſerver, and an able calculator, finds the latitude of Liverpool to be 53° 22′; and its longitude is generally thought to be three degrees weſt of Greenwich, but he believes it to be ſomewhat leſs.

The clock being duly adjuſted by our meridian line, at noon, and the time being found by obſervations of ſeveral ſtars in the evening of March 17, the apparent time of the beginning of the Moon's eclipſe was obſerved to be at $10^h\ 27'$ p. m. and the end at $13^h\ 11'$.

On the next day, I calculated the time of the ecliptic conjunction of the Sun and Moon for April 1, by Meyer's tables, as we have them publiſhed by Mr. Maſkelyne, and then made a projection of the Sun's eclipſe for that time by them, for this place, according to it's latitude as determined by Mr. Holden, and ſuppoſing it's longitude to be 3 degrees weſt from Greenwich; and put up this projection in the council-room, that it might be ſeen, in order to find how it might agree with obſervation.

Being provided with a good reflecting teleſcope at captain Hutchinſon's, I cut a round hole in a paſteboard which would go tight on the tube, and took the Sun's image on a paper behind it, as large as I could have the image of the Sun ſharp and well defined around the edge, which was included in a circle of 4 inches diameter. I divided the diameter into 12 equal parts, for digits, and each digit into 4 parts,

the

the half of every fourth part being left to be eſtimated by the eye.

Mr. Holden and two other gentlemen, who are eſteemed good obſervers, and were provided with refracting teleſcopes and Hadley's quadrants, were with me on Sunday morning, and I deſired a third gentleman to note down the times, and to be careful not to miſtake the minutes of time; as one might be more apt perhaps to miſtake the minutes than the ſeconds. The clouds threatened us diſappointment till about ten minutes before the calculated time of the beginning of the eclipſe, and then the Sun ſhone out very clear; and during the time of obſervation we were but ſeldom interrupted by thin flying clouds. The firſt and laſt contacts of the Moon and Sun were ſo ſharp and inſtantaneous, that it ſeemed poſſible to determine them within one ſecond of time. Several altitudes of the Sun were taken during the eclipſe, by reflecting the Sun's image from a baſon of treacle; and the quantities eclipſed were plainly viſible on the forementioned image of the Sun on the paper, even to the eighth part of a digit. But the altitudes want yet to be corrected by their reſpective refractions.

Several people came into the room to ſee the eclipſe, ſome of whom were ſubſcribers to my lectures; and I could not well refuſe them admittance. But I told them before-hand that they muſt neither ſpeak nor move till the eclipſe was found to be begun. This they ſtrictly complied with, and gave no manner of diſturbance; and, after it was begun, I deſired them all ſeparately to come and view it by the teleſcope, which hindered me from obſerving the number of digits eclipſed for the firſt hour.

I kept

I kept by the reflecting telescope, and watched the Sun carefully for about five minutes before the calculated time of the beginning of the eclipse. Our watches were adjusted to the mean or equal time; and two of them kept exactly alike during the whole time of the eclipse. The observations were as follows:

h	′	″	Digits.	Sun's altitude.
8	59	0	eclipse begun	28° 37′ 00″.
10	2	0	— $8\frac{1}{2}$	
10	5	0	— 9	— 35 49 30
10	11	0	— $9\frac{3}{4}$	
10	13	0	— 10	— 36 29 0
10	18	0	— $10\frac{3}{8}$	
10	21	0	— $10\frac{1}{2}$	— 37 9 0 uncertain.
10	25	0	— $10\frac{1}{2}$	— 37 20 0
10	30	0	— 10	
10	38	0	— 9	
10	40	30	Cusps perpendicular, by a plumb-line's shadow on the Sun's image. Sun's altitude then 38° 48′ 0″.	
10	43	30	— $8\frac{1}{2}$	— 38 57 30
10	47	0	— 8	— 39 8 30
10	54	30	— 7	
11	0	0	— $6\frac{1}{2}$	
11	3	45	— 6	
11	12	0	— 5	
11	15	45	— $4\frac{1}{2}$	
11	19	15	— 4	
11	28	0	— 3 uncertain, on account of a thin flying cloud.	
11	35	2	— 2	
11	45	15	— $0\frac{1}{2}$ uncertain by another cloud.	
11	50	45	eclipse ended; the sky quite clear. Sun's altitude 41° 27′ 0″.	

All wrote down by Mr. Baxtonden, who kept a copy thereof.

At night, Mr. Holden returned and examined the clock by the ſtars, and found the time ſhewn by the clock to be true.

Between the beginning and the middle of the eclipſe, we could plainly perceive inequalities in the Moon's eaſtern limb on the Sun, by means of the reflecting teleſcope; and I often obſerved little tremulous bright ſpecks of the Sun's lowermoſt edge in the otherwiſe dark place juſt before, or weſt, of the lowermoſt cuſp; but they vaniſhed in an inſtant, except one which was conſiderably larger than any of the reſt, and was viſible for about two ſeconds of time by eſtimation: but I was ſo intent upon obſerving it, and looking for others, that I forgot to have the time of its appearance marked down. This undoubtedly was owing to a dent or valley in that part of the limb of the Moon, which no hill beyond it took off from the ſight. But as the eclipſe was drawing toward the end, we could perceive no inequalities of the Moon's weſtern limb on the Sun, nor any ſuch ſpecks in the Sun's edge about either of the cuſps.

As the Moon's latitude was north aſcending, and the cuſps not perpendicular till after the middle of the eclipſe, I apprehend that when they were ſo, the apparent altitudes of the centers of the Sun and Moon were equal. But whether they were then ſo or not, I leave to better judgements to determine.

I ſhall now ſet down the times of the beginning, middle, and ending of the eclipſe, as pre-determined by the above-mentioned projection thereof for Liverpool,

verpool, from Meyer's tables, which were the apparent times; and ſhall reduce the obſerved equal times to the apparent, by ſubtracting 3 minutes 48 ſeconds (which we ſuppoſe here was the equation of time) from the equal times as obſerved by the clock and two watches which kept equally going together.

	Apparent times.					
	By projection.			By obſervation.		
	h	′	″	h	′	″
Beginning	8	56	0	8	55	12
Middle	10	21	45	not certain.		
End	11	48	0	11	46	57
Duration	2	52	0	2	51	45
Digits eclipſed.	$10\frac{25}{60}$			$10\frac{1}{2}$ exactly.		

We wiſh for the accounts of the obſerved times at the Royal Obſervatory and at London; becauſe, by comparing the difference, and making allowance for the velocity of the penumbra between Liverpool and London, the longitude of Liverpool might be known.

As the obſerved quantity was ſomewhat greater than the projected, as to the digits, and the projection which I gave in, ſome time ago, to the Royal Society from Meyer's Elements, made the lower edges of the Sun and Moon to be very nearly in contact at the time of greateſt obſervation at Greenwich, I am apt to think that the appearance at Greenwich was annular: and am, with the greateſt eſteem,

Reverend Sir,

Your moſt obliged humble ſervant,

James Ferguſon.

XX. *Observations on the Eclipse of the* Sun, April 1, 1764, *at* Brompton-Park: *By Mr.* Samuel Dunn.

Read April 5, 1764.

PART of the instruments which I was provided with for observing this eclipse were, a reflecting telescope 21 inches focus with a micrometer, a refracting telescope 6 feet in length with only two glasses, a double convex object glass, and a double convex eye glass. Two pendulum clocks, and a stop watch to seconds of time. I had taken as much care as possible to adjust the clocks to mean solar time some days before the eclipse, and from several altitudes of the Sun, taken March 30th in the morning and afternoon, concluded the clocks were then exactly with the Sun to a second of time.

March 31st was a cloudy day, and not the least hope of seeing the Sun the day following, 'till near midnight, when it cleared up, and the stars appeared.

April 1st, the Sun rose a little obscured by vapours and thin clouds, which he became more free from as he advanced in altitude.

At $6^h\ 56'$ per clock, I took the altitude of the Sun's centre $2^\circ\ 25'\ 20''$, from which observation the clock was 6 seconds of time before the Sun.

At $6^h\ 59'$ per clock, I took the altitude of the Sun's centre $12^\circ\ 50'\ 45''$, from which observation the clock was 4 seconds of time before the Sun.

At

At 7^h $5'$ per clock I took, the altitude of the Sun's centre 13° $49'$ $55''$, from which obſervation the clock was 4 ſeconds of time before the Sun. And the ſame dav in the afternoon,

At 4^h 23 per clock, I took the altitude of the Sun's centre 18° $49'$ $40''$ from which obſervation the clock was 5 ſeconds of time before the Sun.

Theſe and other obſervations confirmed that the clock was 5 ſeconds of time before the Sun at the beginning of the eclipſe.

At 8^h $45'$ per clock, I ſet my watch exactly by the clocks; captain Bentincke and captain Holland were preſent with curious watches.

From 8^h $45'$ per clock, to the beginning of the eclipſe, I continued obſerving with the reflecting teleſcope, and ſaw the limb of the Sun through the teleſcope and thin vapours, without any dark glaſs, as clearly as it could be ſeen by any helioſcope whatſoever.

Captain Bentincke pronounced the ſeconds of time as they were fulfilled by the watch, and as the minutes of time were fulfilled they were written down.

At 9^h $4'$ $29''$ per watch, I thought I ſaw a little dull tremulous vibration obtrude itſelf on the limb of the Sun; and,

At 9^h $4'$ $30''$ it became a little more ſenſible; and,

At 9^h $4'$ $31''$ a little more ſenſible; but it was

At 9^h $4'$ $32''$ per watch before I was certain the Sun's limb was touched by the limb of the Moon; And,

At 9^h $4'$ 33 I plainly ſaw, through this teleſcope, and the thin vapours of the atmoſphere, the leaſt viſible dent, perfectly well defined in the Sun's limb.

 I inſtantly

I inſtantly compared the watch with the clocks, and found the watch had loſt 4 ſeconds of time of the clocks, a property which it had before had, when taken out of the pocket and expoſed to the cold air.

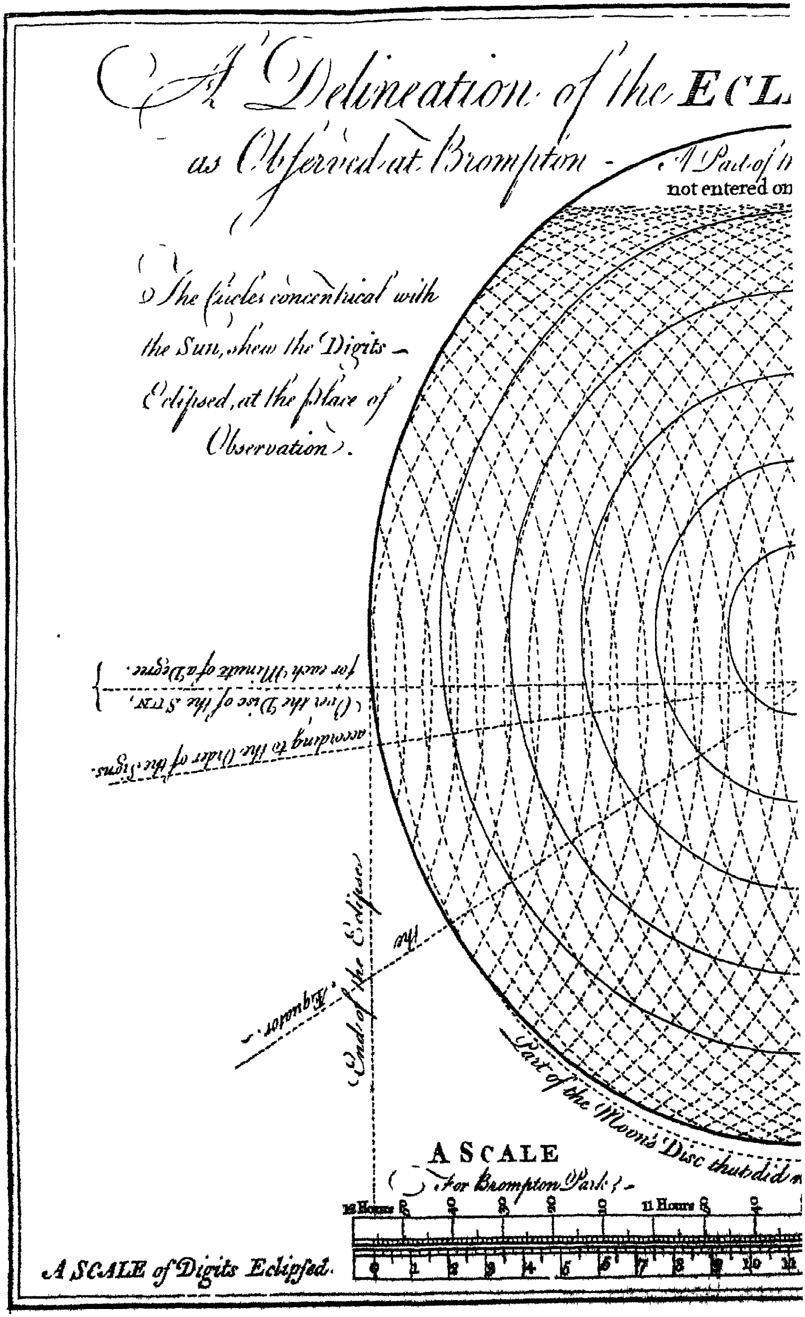
The watch was therefore but one ſecond of time before the Sun at the time of obſervation, for Brompton park, which is exactly one mile from Hyde-park-corner, in the way towards Kenſington.

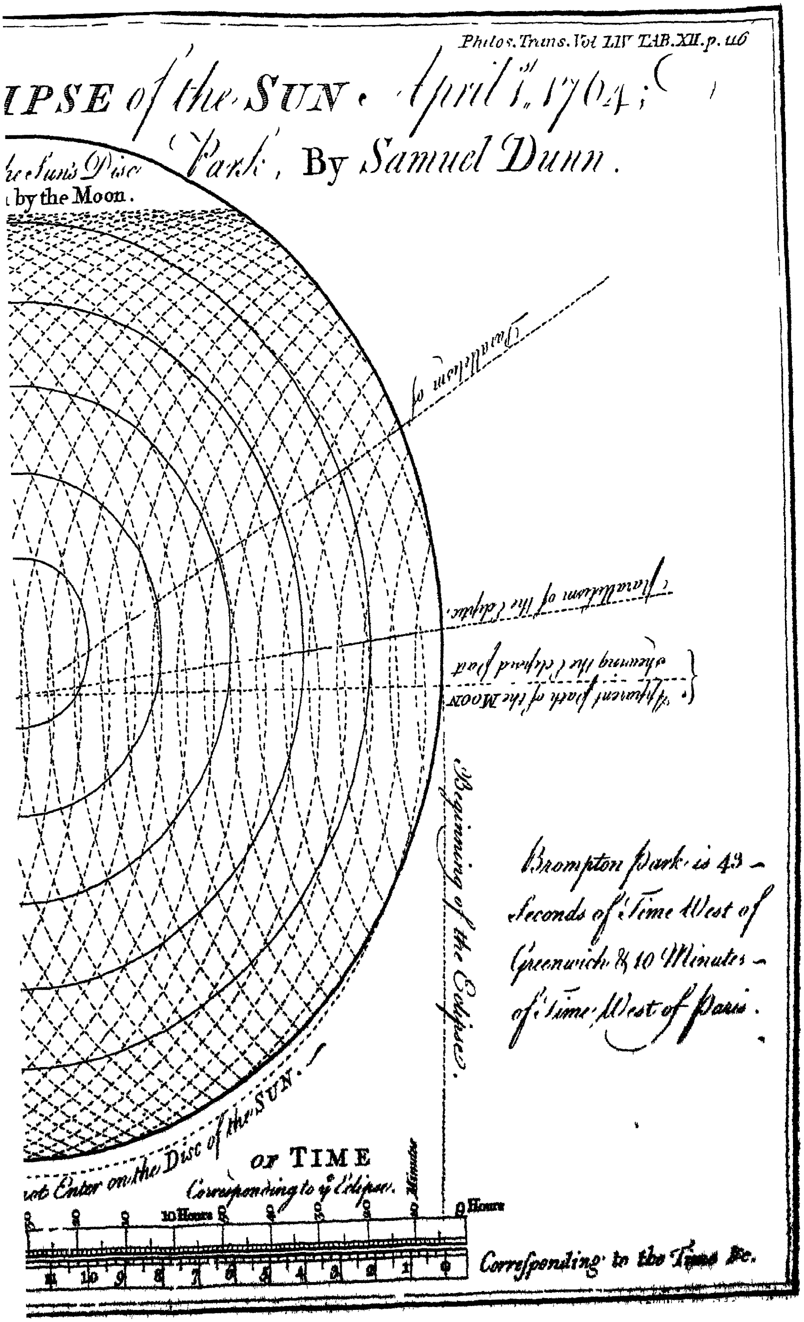
Clouds prevented the end of the eclipſe from being obſerved; at $12^h\ 3'$ the Sun appeared, and the eclipſe was ended, and from the obſervations which I made, have drawn a map of the phaſes and ending, which is herewith, TAB. XII.

London, April 4, 1764.

Samuel Dunn.

Obſervations


A Delineation of the ECL
as Observed at Brompton - A Part of
not entered on
The Circles concentrical with the Sun, shew the Digits Eclipsed, at the place of Observation.
for each Minute of a Degree.
Over the Disc of the SUN,
according to the Order of the Signs.
the Equator.
End of the Eclipse
Part of the Moon's Disc that did
A SCALE
For Brompton Park.
12 Hours 50 40 30 20 10 11 Hours 50 40
A SCALE of Digits Eclipsed.
0 1 2 3 4 5 6 7 8 9 10 11


Philos. Trans. Vol LIV TAB. XII. p. 116
IPSE of the SUN. April 1. 1764;
Park, By Samuel Dunn.
he Sun's Disc
by the Moon.
Parallelism of
Parallelism of the Ecliptic.
Apparent Path of the Moon
shewing the Eclipsed Part
Beginning of the Eclipse.
Brompton Park is 43 Seconds of Time West of Greenwich & 10 Minutes of Time, West of Paris.
not Enter on the Disc of the SUN.
OF TIME
Corresponding to ye Eclipse.
Minutes
10 Hours
0 Hours
Corresponding to the Time &c.

Observations on the Eclipse of the Moon, 17*th* March 1764, *made at* Brompton-Park, *near* London, 10′ *of Time West of* Paris, *and* 43″ *of Time West of the* Royal Observatory *at* Greenwich: *By Mr.* Samuel Dunn.

Solar time. h	′	″	
At 10	39	30	the eclipse begins in that part of the Moon's limb between Tycho and Grimaldus.
— 10	40	30	Skikardus toucheth the shade.
— 10	45	0	the lower part of Mare Humorum is touched by the shade.
— 10	51	10	Tycho and Grimaldus touch the shade.
— 10	53	0	Grimaldus exactly covered and Tycho a little immerged in the shade.
— 11	7	15	Galileus toucheth the shade.
— 11	11	45	Keplar and Lansbergius touch the shade.
— 11	18	10	Copernicus toucheth the shade, and the lower part of Mare Nectaris is a little immerged.
— 11	28	15	Ariadeus toucheth the shade, in Mare Tranquillitatis
— 11	36	30	Julius Cesar toucheth the shade.
— 11	41	0	Manilius toucheth the shade, and Plinius not yet immerged.
— 11	47	10	Mare Crisium it's lower part toucheth the shade.
— 12	15	40	Grimaldus is quite immerged.
— 12	17	45	Keplar emerges from the shade.
— 12	20	30	Copernicus emerges.
— 12	34	30	the line of the shade passeth between Julius Cesar and Manilius, and at the same time a little of Mare Humorum is emerged.
— 12	46	25	Mare Crisium begins to emerge, and at the same time the line of the shade passeth a little below Plinius and Vitruvius in Mare Tranquillitatis.
— 12	56	50	Tycho emerges.
— 12	57	20	the centre of Tycho is a little emerged, and the line of the shade passeth through Mare Crisium two thirds from the upper end.
— 13	1	50	Mare Crisium is quite emerged.
— 13	5	45	Theophilus in Mare Nectaris emerges.
— 13	22	10	the eclipse ends in that part of the Moon's limb cut by a line drawn through the Moon's centre and the lower end of Mare Nectaris.

Note, In the above observations, where it is said that any spot touched the shade, it is meant, that that spot was centrally bissected by the line of the shade at that time.

XXI. An

XXI. *An Account of the Degree of Cold observed in* Bedfordſhire: *By* John Howard, *Eſq; F. R. S. in a Letter to* John Canton, *M. A. F. R. S.*

S I R,

Read April 12, 1764.

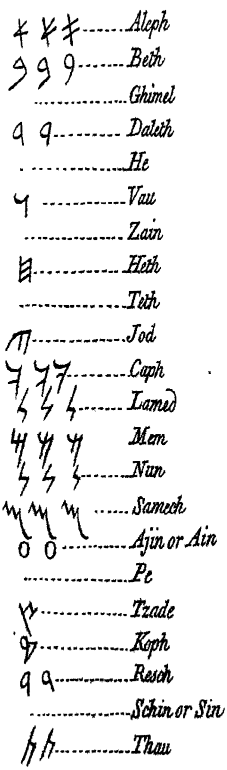
I Would beg leave to acquaint you of a degree of cold that I obſerved at Cardington, in Bedfordſhire, the 22d of November laſt: juſt before Sun riſe Farenheit's ſcale by one of Bird's thermometers being ſo low as 10 and $\frac{1}{2}$. If it will throw any light on the locality of cold, or think it worth the Society's obſervation, would leave to your better judgment, and remain with great eſteem,

S I R,

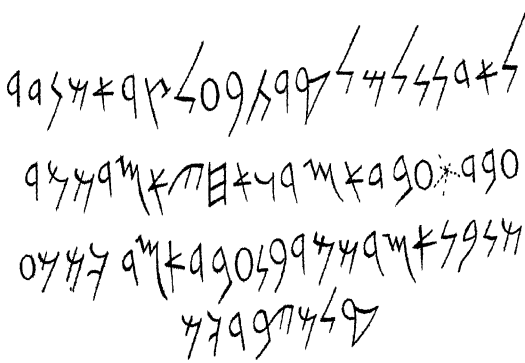
Your moſt obedient ſervant,

John Howard.

The Phoenician *Alphabet, deduced from two Transcripts of the* Maltese-Phoenician *Inscription*



The Maltese-Phoenician *Inscription.*



J. Mynde sc.

The following paper was received a little before the recess of the Royal Society, in 1763; *but was afterwards mislaid, and not found 'till the beginning of* April 1764. *To this accident is solely to be imputed the delay of it's publication.*

XXII. *Some Remarks upon the first Part of M. l'Abbé* Barthelemy's *Memoir on the* Phœnician *Letters, relative to a* Phœnician *Inscription in the Island of* Malta. *In a Letter to the Rev.* Thomas Birch, *D. D. Secret. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Member of the Academy* degli Apatisti *at* Florence, *and of the* Etruscan *Academy of* Cortona *in* Tuscany.

Good Sir,

Read April 12, 1764.

M. l'Abbé Barthelemy having lately communicated to the learned world (1) a copy of one of the Phœnician inscriptions long since discovered in the island of Malta, more accurately taken (as he pretends) than any of those that had ever before appeared, and attempted to ex-

(1) *Mem. dans lequel on prouve, que les Chinois sont une colonie Egyptienne*, par M. de Guignes, &c. p. 39—54. A Paris, 1760.

plain

plain it in a manner perfectly new; I shall beg leave to make a few cursory remarks upon what he has been pleased to advance, on this occasion. Which remarks may perhaps be deemed not altogether unnecessary, as part of the inscription, according to M. l'Abbé's lection of it, seems at least somewhat involved, if not wholly unintelligible; and consequently will admit, unless I am greatly deceived, of a farther illustration. Nor can M. l'Abbé be disgusted at my presuming to differ in a few particulars from him, as he is not acted by a spirit of ostentation, or a thirst after applause, but the love of truth; and as he has taken the same liberty with one of my dissertations, which the Royal Society did me the honour to publish a few years since, upon a similar subject.

I.

M. l'Abbé observes, that "in (2) the beginning "the Phœnician letters were not distinguished from "the Samaritan; but that most of them in process "of time admitted of such great variations, that the "traces of their origin are very frequently lost." Hence it seems to appear, that the later any Phœnician inscriptions are, the more the forms of their letters must recede from those of the correspondent Samaritan; and, vice versâ, that the more remote the ducts of any Phœnician elements from those of the correspondent Samaritan are, the later such characters, and consequently the inscriptions formed of them, must be. Let this be allowed, and it will be sufficiently manifest, that the Maltese inscription (which

(2) Ibid. p. 39.

our

our author ſeems to have had principally in view, when he made the preceding obſervations) is ſeveral years at leaſt poſterior to the days of Simon, prince and high-prieſt of the Jews. The alphabet therefore deduced from this inſcription, which differs pretty conſiderably from that exhibited by the ſepulchral ſtones found in the ruins of Citium, ought not to paſs for the true antient Phœnician alphabet, that prevailed over ſo great a part of the Eaſt in the earlier ages.

From the paſſage here produced we may farther infer, that the Phœnician inſcriptions either coeval with the Samaritan coins ſtruck by Simon, prince and high prieſt of the Jews, or older than thoſe coins, muſt be formed of letters, for the moſt part, extremely ſimilar to the Samaritan. And this we find in fact to be true. Many of the elements therefore of thoſe inſcriptions may be more eaſily diſcovered by the aſſiſtance of Simon's medals, than by that of any monument of antiquity ſeveral hundred years later. Nay, the powers of many letters, allowed to belong to the Phœnician alphabet, have been actually aſcertained by means of the correſpondent elements on the Samaritan coins. We muſt not therefore too haſtily admit, or too cloſely adhere to, what M. l'Abbé has been (3) pleaſed to lay down, in the moſt unlimited terms, as a certain and indubitable truth; viz. that "a Phœnician alphabet ought by no means to "be founded upon the affinity of its letters with thoſe "of other alphabets." The alphabets he himſelf has given us, incomplete as they are, will be conſidered as a ſufficient refutation of this aſſertion.

(3) Ibid. & Planch. I.

What has been here obſerved of the Malteſe inſcription is, with regard to it's antiquity, at leaſt, equally applicable to that of Carpentras (4), the ducts of whoſe letters are ſtill more remote from thoſe of the Samaritan; and conſequently this, according to M. l'Abbé's deciſion, muſt be ſtill of a later date. And, indeed, the rude and almoſt barbarous forms of it's Phœnician elements render this inconteſtably clear. Some of them are extremely ſimilar to, if not apparently the ſame with, thoſe of the correſpondent letters on certain Spaniſh or African Phœnician coins, ſtruck, as there is reaſon to believe, after the commencement of the Roman empire. And if this be the caſe, how can we ſuppoſe them to have been ſome of the firſt alphabetic characters that ever appeared, or thoſe immediately deduced (5) from hieroglyphics themſelves? They ſeem to have been only corruptions of the earlier Phœnician letters, from whoſe forms ſeveral of them have very conſiderably varied. So high an antiquity as that above ſuppoſed is not announced by the face of the inſcription, and therefore the learned will not perhaps readily aſſent to ſuch a ſuppoſition.

II.

The eleventh letter of the firſt line, which is taken for *Thau*, ſeems to have been a little mutilated by the injuries of Time; as part of the ſtrait line cutting

(4) Ibid. p. 53.

(5) *Recueil d'Antiquit. Egypt. Etruſq. Grec. & Romain.* par M. le Comte de Caylus, Tom. I. p. 65. Plan. xxvi. A Paris, 1752.

the perpendicular, in this element, is effaced. That this is fact, appears from two tetradrachms of Menæ, an antient town of Sicily, at present going under the denomination of Menéo, now in my hands; on one of which the whole figure of this kind of *Thau* is perfectly well preserved, and on the other a similar figure is visible, though the original transverse line has been somewhat diminished. One of (6) Lord Pembroke's medals of the same town also presents to our view a *Thau* completely formed. We meet with this letter in the Citiean inscriptions, sometimes as it has been handed down to us by the Punic Medals of Sicily, and sometimes as it is represented in M. l'Abbé's plate of the Maltese inscription; part of it perhaps having been lost, in the course of so many ages. However, that the earlier Phœnician *Thau* frequently bore some sort of resemblance to the character taken for the same element on the Sicilian coins, there is great reason to believe; since otherwise it could not have resembled a cross, as it most certainly did. For that the antient Samaritan *Thau*, nearly agreeing in figure with the Phœnician, had the appearance of a cross, we learn from some good (7) authors. In the later periods, and perhaps to the time when the Phœnician alphabet itself began to be disused, it might in certain countries, pretty remote from Phœnicia, have assumed a somewhat different form, though this I must not pretend absolutely to affirm; but that the inscription I am considering exhibited at

(6) *Num. Ant. &c.* à Thom. Pemb. et Mont. Gomer. Com. *Collect.* P. 2. T. 87. Lond. 1746.

(7) Tertullian. Hieronym. &c. Vid. Val. Ern. Loescher. *De Cauf. Ling. Eb.* p. 234. Francofurti & Lipsiæ, 1706.

firſt the complete figure of *Thau*, (8) from P. Lupi's draught of it, taken upon the ſpot, in 1735, I think we may fairly preſume. And that the *Thau*, as it there appears, was not ſeldom uſed in the earlier Carthaginian times, from the medals of Menæ already touched upon, to omit others that might be produced, is, I conceive, inconteſtably clear.

III.

I cannot forbear ſuſpecting, that M. l'Abbé has a little deviated from the genuine form of the *Aleph* in his plate; notwithſtanding the accuracy with which, as he informs us, Count Caylus's copy was taken. That Phœnician element occurs upon my Punic and Phœnician coins, not to mention thoſe of my friends, above thirty times; and yet not one of theſe characters exhibits an angle, formed of two right lines cutting the perpendicular, as does the *Aleph* here. Nor do we meet with ſuch a figure of *Aleph* in the Citiean inſcription, preſerved on the original ſtone, brought from Cyprus by Dr. Porter, and preſented to the Univerſity of Oxford by Charles Gray, Eſq; member of Parliament for Colcheſter, and fellow of the Royal Society; though the uſual form of this letter is found, oftener than once, in that inſcription. P. Lupi's copy (9) of the Malteſe inſcription exhibits the *Aleph* (whoſe ducts were perhaps better preſerved when that tranſcript was taken than at the time Count

(8) Sig. Ant. Fran. Gor. *Diſeſ. dell' Alphab. Etruſc.* p. 102. & Tab. III. p. 109. In Firenze, 1749. Lup. *Letter. Philolog.* Let. XI. p. 6.

(9) Gor. *Diſeſ. dell' Alphab. Etruſc.* & Lup. ubi ſup.

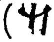
Caylus's

Caylus's copy was ſent him) as it appears on my Punic and Phœnician coins.

IV.

The eighteenth letter of the firſt line of this inſcription is not *He*, as M. l'Abbé ſuppoſes, but *Mem*. This is rendered indiſputable by the form of the element itſelf, as well as by the tenor of the inſcription. That the form of the element perfectly reſembles, or rather is altogether the ſame with, that of *Mem*, is ſufficiently evident from M. l'Abbé's own plate, and even more ſo from the copy communicated to Sig. Gori by P. Lupi. And with regard to the tenor of the inſcription, I ſhall not ſcruple to affirm, that this abſolutely requires the letter to be *Mem*, and not *He*. For otherwiſe the word צרא, TZORA, or TZVRA, muſt denote Tyre; whereas the name of that city in Syriac, ſuppoſed by M. l'Abbé to be the language of the inſcription, as well as Chaldee, Hebrew, Samaritan, and Arabic, is צר, TZOR, or TZVR. To which we may add, that the four laſt elements of this line cannot form the word הנדר, HOC VOTVM, THIS VOW, as M. l'Abbé aſſerts, becauſe that would be neither Syriac nor ſenſe; as the monument could not with any manner of propriety be termed a vow, and conſequently could not be tranſmitted down to future ages under the denomination of THIS VOW. It was not a vow, but erected in purſuance of a vow. The eighteenth and the three preceding letters then form the words צר אם, TYRVS MATER, or rather here TYRI MATRIS, OF TYRE THE METROPOLIS, as we find that city

city actually ſtyled on ſeveral Tyrian coins. This ſenſe is perfectly conſonant to the tenor of the inſcription, and will therefore, I flatter myſelf, be readily admitted by (10) M. l'Abbé.

But to put the point here inſiſted on even beyond the poſſibility of a doubt, I ſhall beg leave farther to obſerve, that I have two of thoſe Phœnician coins (11) attributed by M. l'Abbé to the city of Marathus; one of which exhibits the firſt letter *Mem* as it ſtands in his alphabet, () and the other exactly as he has preſented to our view the pretended form () of *He*. Such demonſtration as this muſt convince every one, that is not reſolved to be proof againſt conviction, and be allowed abſolutely deciſive in this affair.

V.

Though M. l'Abbé has not inſerted the laſt letter of the word עבדו, in the beginning of the ſecond line, that element was nevertheleſs moſt certainly *Vau*. This is ſufficiently manifeſt from P. Lupi's (12) copy, as well as from the vacant ſpace after *Daleth*, which is capable of containing only a ſingle letter, and that can be no other than *Vau*. Beſides, יברכם, BENEDICAT ILLIS, not NOBIS, MAY HE BLESS, or PROSPER, THEM, not US, the laſt word of the inſcription, ſets this point in the cleareſt light. The next word but one, ואחי, has been rendered by M. l'Abbé Barthelemy ET FRATER MEVS, though ET FRATER EIVS muſt be allowed much more con-

(10) *Journal des Sçavans*, Aout 1760. p. 267–271.
(11) Ibid. p. 275.
(12) Gor. & Lup. ubi ſup.

ſonant to the tenor of the inſcription. I am therefore inclined to believe, that the word ואחי ought to be conſidered as in regimen, or conſtruction; and that the proper name עבדאסר, ABDASAR, is underſtood after the laſt mentioned term. Inſtances of ſuch an ellipſis as this occur, in PSAL. lxxiv. 19. ISAI. xiv. 6. &c. and have been produced by Buxtorf, in the piece (13) referred to here. To which we may add, that the perſon of the verb עבדו ſtrongly countenances, at leaſt, if it does not give an abſolute ſanction to, what has been here propoſed to the conſideration of the learned.

VI.

The proper name עבדאסר, in the ſecond line of the inſcription, was pronounced, as I apprehend, by the orientals ABDASAR, or ABDESER, not ABDASSAR. This is rendered not a little probable by the proper names ASAR-HADDON and TIGLATH-PIL-ESER, the *Samech* neither in ASAR nor ESER there having received a dageſch from the Maſorites. The ſame (14) point is alſo confirmed by the Septuagint. It likewiſe appears from the Etruſcan ÆSAR, which is evidently the ſame word with the Phœnician and Chaldee ASAR, or ESER, and in common with it denotes GOD. That ASAR, or ESER, in general ſignified GOD, is allowed by a (15) learned writer;

(13) Johan. Buxtorf. *Theſaur. Grammat. Ling. Sanct. Hebr.* p. 363. Baſileæ, 1663.

(14) 2 KING. xv. 29. xix. 37. ISAI. xxxvii. 38. EZR. iv. 2. Vid. etiam Matth. Hiller. *Onomaſt. Sacr.* p. 607, 608.

(15) Matth. Hiller. *Onomaſt. Sacr.* p. 596. Tubingæ, 1706.

though

though that it was alſo applied to one particular deity, the words ASAR-HADDON, TIGLATH-PIL-ESER, &c. ſeem clearly to evince. The Gallic HESVS, taken by ſome for the god Mars, of a ſimilar ſound, may perhaps bring an acceſſion of ſtrength to what has been here advanced. The name ASAR, or ESER, therefore amongſt the Phœnicians was probably both equivalent to the general term GOD, and likewiſe pointed out to them one particular deity. In ABD-ASAR, or ABD-ESER, it muſt have anſwered to BACCHUS, or DIONYSVS; the whole name correſponding with ΔΙΟΝΥΣΙΟΣ, BACCHICVS, OF or BELONGING TO BACCHVS, THE SERVANT OF BACCHVS, the true import of the two words ABD-ASAR, or ABD-ESER, of which this Phœnician proper name is compoſed.

VII.

With regard to the next proper name, אסרממר, אסרמ־מר, or rather אסרם־המר, ASERIM-HAMMAR, M. l'Abbé and I differ conſiderably in our notions. He takes the fourth letter for *He*, and I for *Mem*. Now this character is exactly the ſame with that in the firſt line of the inſcription, which I have already proved to be *Mem*, and conſequently it muſt be conſidered as altogether the ſame element. Admit this, and the two words forming the proper name, אסרם־מר, or rather אסרם־המר, will immediately occur. I ſay אסרם־מר, or rather אסרם־המר, becauſe ſuch an ellipſis of the prefix (particularly (16) in the proper name ענם־מלך, or ענם־המלך) may ſometimes be

(16) Id. ibid. p. 603, 608.

obſerved.

observed. In fine אסרם־מר, or אסרם־המר, must be composed of אסרם, ASERIM, (the Phœnician name of one of the kings of Tyre, (17) according to Menander Ephesius) and המר, HAMMAR, IPSE DOMINVS; the term מר, MAR, entering into the composition of certain similar (18) names. Now ASERIM-HAMMAR being equivalent in Phœnician to the Greek, or rather Egyptian, SARAPION; the deity denominated SARAPI, or SARAPIS, in Egypt, must have assumed the name of ASERIM at Tyre, as from the inscription now before me may be very fairly inferred.

VIII.

That the first letter of the third line is *Mem*, and not *He*, as M. l'Abbé is pleased to affirm, both the form itself and the sense of this part of the inscription seem evidently to prove. The figure here is altogether the same with that of the eighteenth element in the preceding line, and consequently must be allowed to represent *Mem*. The sense also evinces this, beyond the possibility of a doubt. For, I believe, it will be no easy matter to prove the reality of M. l'Abbé's word הנבן, or at least to render it intelligible here. But with respect to the word מן, which I take the two first letters to form, equivalent in Syriac to the (19) Latin QVI (est), or IS QVI (est), it

(17) Menand. Ephes. apud Joseph. *Cont. Apion.* Lib. I.
(18) Matth. Hiller, ubi sup. p. 602, 603.
(19) Buxtorf. *Lex. Chald. & Syriac.* p. 301, 302.

clearly continues the ſenſe, and is of courſe moſt perfectly conſonant to the tenor of the inſcription.

IX.

What has been obſerved of the laſt mentioned character is equally applicable to the eighth and twentieth elements of the ſame line. They are both indubitably *Mem*. This, not only by the powers of the letters with which they are immediately connected, but likewiſe by what has been already advanced, is rendered indiſputably clear. 'Tis worthy obſervation, that M. l'Abbé Fourmont was the firſt (20) who took this character for *He*; and pretended, abſurdly enough, to deduce it from the Eſtrangelo form of that element.

X.

With regard to the laſt word but one of the inſcription, כממעקלם, or כממעקלים, it ſeems to be compoſed of the inſeparable particles, מ, כ, and the participle מעקלם, or מעקלים, TORTVOSI, INFLEXI, TORTE NAVIGANTES, or HVC ET ILLVC AGITATI. This therefore may be appoſitely enough rendered, TANQVAM EX IIS QVI HVC ET ILLVC AGITANTVR, INFLEXO CVRSV (vel ITINERE) JACTANTVR, TORTE NAVIGANT, &c. It may alſo be tranſlated, with ſufficient propriety, QVVM HVC ET ILLVC AGITATI fuerint, QVVM TORTE NAVIGAVERINT, &c. WHEN THEY SHALL BE TOSSED (upon

(20) *Saggi di Diſſertazion. Accademich. &c. di Corton.* Tom. III. p. 90. In Roma, 1741.

the

the sea) HITHER AND THITHER, WHEN WITH MANY TURNINGS AND WINDINGS THEY SHALL PLOUGH THE OCEAN, &c. the particle ב sometimes denoting QVVM, and a pleonasmus or redundancy of מ having not been antiently uncommon, according to (21) Noldius.

The two Maltese stones therefore adorned with this inscription, similar to many others with which we are supplied by the remains of antiquity, must be considered as votive monuments. They were erected by Dionysius and Sarapion, both Tyrians, and the sons of Sarapion, in consequence of a vow, to Hercules, surnamed the CONDUCTOR, or the CHIEF CONDUCTOR, for a prosperous voyage. That the inscription runs in the third person, not the first, as M. l'Abbé Barthelemy asserts, from the correspondent Greek one, and the considerations already offered in support of this point, seems sufficiently clear. This the last word יברכם, BENEDICET ILLIS, not NOBIS, HE WILL BLESS THEM, not US, or BENEDICAT IPSIS, MAY HE BLESS THEM, not US, must be allowed also indubitably to prove.

XI.

If the preceding observations should meet with the approbation of the Royal Society, the following translations of the inscription now before me may perhaps not prove unacceptable to the learned. [See the Inscription, TAB. XI.]

(21) Christ. Nold. *Concordant. Particular. Ebræo-Chald.* p. 353, 354, 470. *Jenæ*, 1734.

לארנן למלקרת בעל צר אם נדר
עברו עבדאסר ואחי אסרממר
מן בן אסרממר בן עבדאסר כממע
קלם יברכם

DOMINO NOSTRO MELCARTHO DEO (tutelari) TYRI
MATRIS VOTVM
FECERVNT ABDASARVS ET FRATER (ABDASARI)
ASERIM—HAMMARVS
IS QVI FILIVS (eſt) ASERIM-HAMMARI FILII AB-
DASARI—IIS
TORTE NAVIGANTIBVS (vel HVC ET ILLVC INFLEXO
CVRSV AGITATIS) BENEDICAT.

ABDASAR AND HIS BROTHER ASERIM-HAMMAR WHO (alſo) IS THE SON OF ASERIM-HAMMAR THE SON OF ABDASAR HAVE MADE A VOW TO MELCARTHUS (or HERCULES) THE (tutelary) GOD OF TYRE THE METROPOLIS—IN THEIR TURNINGS AND WINDINGS (or IN THEIR CROOKED NAVIGATION) MAY HE BLESS (or PROSPER) THEM.

That M. l'Abbé Barthelemy's explication of the inſcription here conſidered is at leaſt ſomewhat involved, from the foregoing remarks, ſeems ſufficiently clear; whether or no his obſcurity, or miſtakes, if any ſuch there be, are removed by the illuſtration now offered, the Royal Society will be the beſt able to decide.

But though M. l'Abbé has perhaps not arrived at a complete interpretation of this inſcription, he has nevertheleſs thrown much more light upon it than either

either M. le Commandeur (22) de Marne, or M. l'Abbé Fourmont, or indeed any other person who attempted, before his memoir was read, an explication of it. This cannot but be acceptable to the lovers of antiquity, and must intitle him to the thanks of the learned world.

XII.

The language of the inscription is a mixture of Hebrew and Syriac. The first word, לארנן, must be looked upon as Syriac; as may likewise the sixth term, נדר, on account of the sense wherein it is used. The seventh is Hebrew, as well as Syriac. The ninth, however we render it, is undoubtedly Hebrew; and the eleventh, however this may have escaped M. l'Abbé, as certainly Syriac. The two last words are manifestly Hebrew, though in the last syllable of the former of them *Jod* is suppressed. But this is intirely consonant to the Phœnician form, the coins struck at Sidon generally exhibiting לצדנם, for לצדנים, with the *Jod* expunged. The term בן, SON, used twice in this inscription, is here also apparently Hebrew. We cannot therefore infer from the monument under consideration, as (23) M. l'Abbé Barthelemy has done, that "there is scarce any "difference at all between the Phœnician and Syriac "languages." Nor will M. (24) de Guignes merit

(22) *Saggi di Dissertazion. Accademich. &c. di Corton.* Tom. I. Par. I. p. 25—34. In Roma, 1735. & Tom. III. p. 89—111. In Roma, 1741.

(23) M. de Guign. ubi sup. p. 47.

(24) Id. ibid. p. 60.

rit the attention of the learned, when he is pleaſed to aſſert, that " M. l'Abbé Barthelemy has actually " proved, from this inſcription, that the Phœnician " language is nothing elſe but the Syriac tongue."

XIII.

As the moſt antient Phœnician language was almoſt intirely the ſame with the (25) Hebrew, the Syriac words that occur in this inſcription, together with what has been already remarked of the forms of the letters it contains, announce it to have been of a later date. The figure of the *Koph* in particular agrees in all reſpects with the form of the ſame element exhibited by a coin ſtruck at Achola, or Achulla, as the name appears on this medal, in the Auguſtan age. That at this time, and even earlier, as well as later, ſeveral Syriac words ſhould have been uſed by the Phœnicians of Tyre, can be no matter of ſurprize, when we conſider, that the Jews themſelves, during this period, ſpoke a language extremely ſimilar to, if not almoſt intirely the ſame with, the Syriac.

XIV.

I muſt beg leave farther to remark, that, by the aſſiſtance of the monument now before me, two Phœnician proper names have been diſcovered, which have never hitherto in any of the antient hiſtorians occurred. As the *Aleph* in אסר was, however, ſometimes pronounced like E, and perhaps I, the word

(25) Bochart. *Chan.* Lib. II. cap. 1.

ABDISSAI

ABDISSAR, (26) met with by M. l'Abbé Barthelemy on an antient coin, and ABDASAR, exhibited by the Maltese stones, may by some possibly be considered as nearly the same name. Should this prove really the case, M. l'Abbé must be allowed to have been extremely lucky in meeting with a proper name so similar to, or rather scarce distinguishable from, one preserved in an inscription, he was just going to explain. Be this as it will, the word ABDASAR appears, as a part of another Phœnician inscription, on a piece of marble, found amongst the ruins of Citium; which was presented by Charles Gray, Esquire, member of Parliament for Colchester, and fellow of the Royal Society, a gentleman of great merit and erudition, to the University of Oxford.

XV.

To what has been here advanced it may not be improper to subjoin an alphabetic table of the Phœnician letters forming the Maltese inscription, which M. l'Abbé Barthelemy has lately attempted to explain; [Vide TAB. XI.] and on which, in this paper, I have been endeavouring to throw some additional light.

The form of the *Thau* in the table, not bearing the least resemblance to a cross, approaches pretty near that of *Tzade* (27), as exhibited by several of my

(26) *Mem. de Litter. &c.* Tom. XXVIII. p. 597. A Paris, 1761.

(27) Of all the letters in the Phoenician alphabet none perhaps has a greater variety of forms than *Tzade*. One of these, that not seldom occurs upon the Tyrian and Sidonian coins, pretty much resembles the character which M. l'Abbé Barthelemy takes

Tyrian

Tyrian and Sidonian coins; though these characters, as upon inspection will appear, are sufficiently distinguishable from each other.

for *Thau*, in the Phœnician inscription here explained. Now this very figure of *Tzade* immediately precedes the numeral characters in the exergues of several Sidonian coins, and is itself immediately preceded by the letter *Schin*. Those two elements therefore, as occurring on the medals of Sidon, and preceding a date, I took for the initial letters of the words שנת צדן, THE YEAR OF SIDON; and evinced this by such reasons as, I apprehended, could not be easily overthrown. But M. l'Abbé Barthelemy believes the two elements to form the word שת, YEAR; and has been followed in this notion by M. Pellerin, who seems a little to exult, and triumph, on the occasion. However, I still am fully convinced of the truth of what I formerly advanced; and am hindered from coming into M. l'Abbé's opinion, by the following considerations.

1. The very character I took for *Tzade* is the first letter of the words צר, TZOR, or TZVR, and צדן, TZIDON, TYRE and SIDON, on several Tyrian and Sidonian coins. This directly evinces the point in question. Some of these coins are now in my possession, very well preserved, and undoubtedly genuine.

2. The word שת, ANNVS, YEAR, or THE YEAR, does not occur in any of the oriental languages, or dialects, that I have hitherto been conversant with; the term שתא, which is rarely used, being a different word. Nor can M. l'Abbé authenticate the pretended שת, by observing, or rather without any foundation roundly asserting, that as from בנת is deduced בת, so from שנת may be derived שת, SCHAT. For we certainly know, that there is such a word as בת, whereas שת has not hitherto been found. We are not at liberty to frame terms out of our own heads, in order to serve an indefensible hypothesis; nor will a grammatical conjecture, as I apprehend, realize a non-entity.

3. The numbers expressed by Phœnician numeral characters on certain coins that indisputably belong to Sidon do not amount to 112. From whence, as I formerly observed, we may infer, that the æra referred to by those coins was the later epoch of Sidon. Which if we admit, any year deduced from that æra

The

The figure of the *Koph* here is by no means the most antient representation of that element. It

may, with the utmost propriety, be styled THE YEAR OF SIDON; and that appellation may be more naturally supposed to be pointed at by the letters *Schin* and *Thau*, prefixed to the dates on the small Sidonian coins, than the single word at length denoting THE YEAR.

4. On one or two medals in my possession, the element *Schin*, as the initial letter of שנת, THE YEAR, OR IN THE YEAR, not the whole word denoting THE YEAR, appears before the numeral characters in the exergue. From whence it seems clearly to follow, that the elements preceding those characters ought to be taken for initial letters, and consequently that the character I denominate *Tzade* ought to be considered in that view.

5. The year exhibited by the first medal in my [a] plate of Phoenician coins, allowed by M. l'Abbé Barthelemy himself to belong to Sidon, and adorned with a numeral inscription, denoting THIRTY-SIX, cannot be the thirty-sixth year of the æra of Seleucus, because the Sidonians were then subject to Ptolemy Philadelphus, king of Egypt, in whose territories the supputation according to that epoch did not take place. The æra then to which the date on this coin refers must undoubtedly be the proper æra of Sidon, which commenced in the year of Rome 643. Nothing therefore can be more natural than to suppose, that the two alphabetic characters preceding the numeral inscription should be the initial letters of the words שנת צדן, THE YEAR OF SIDON, as I formerly ventured to suggest. From whence it seems also clearly to follow, that the notation by me considered in a former paper did not prevail at Sidon before the 207th year of the æra of Seleucus, nearly coincident with the 643d of Rome.

6. If what has been here remarked should meet with the approbation of the learned, they will perhaps not so readily admit M. l'Abbé Barthelemy's pretended coins of Marathus to have appertained to that inconsiderable city. Besides, should this be allowed, which, I am persuaded, it will not, the notation they exhibit cannot well be supposed to have been introduced there before the later Sidonian æra commenced. For if the numeral

[a] See *Philosoph. Transact.* Vol. L. P. ii. Tab. XXXI. p. 791.

too much reſembles the ſquare or Chaldee *Kopb*. The character denominated *Koppa*, viſible on the

characters above-mentioned were not received in ſo large, opulent, and polite a city as Sidon, before the year of Rome 643; it is utterly improbable, that they ſhould have been uſed at Marathus, or any other obſcure place of Phœnicia, before the commencement of that year. Nay, it is highly probable, that the introduction of them there was poſterior to it. Now if theſe numeral characters were firſt received at Marathus in the year of Rome 643, or rather a little after that year, ſome of M. l'Abbé's pretended coins of Marathus were ſtruck there in the days of Strabo. But then, according to that excellent [b] author, the city was deſtroyed, and it's territory occupied by the Aradians, amongſt whom it was divided by lot; ſo that the foregoing ſuppoſition is, at firſt ſight, manifeſtly abſurd. Farther, the word on theſe medals taken by M. l'Abbé to denote Marathus is frequently not מרתח, as he ſuppoſes, but מרתב, though on ſome few of them part of the laſt letter only appears. This M. l'Abbé, without any manner of foundation, ſeems to think a new form of the *Ajin* [c], and believes it to be the initial letter of the name of a month; though he had before, in a great meaſure at leaſt, exploded the notion of ſuch initial letters. In fine the conſiderations now ſubmitted to the judgment of the learned abſolutely determined me to cancel part of a ſheet of a ſmall work, put to the preſs here, in 1753; wherein I aſſerted, and endeavoured to prove, that thoſe coins belonged to Marathus. This will be atteſted by the workmen I imployed, and the IMPRIMATVR given me by the Rev. Dr. Brown, Maſter of Univerſity College, our worthy Vice-Chancellor, at that time. The cancelled part of a ſheet is ſtill in my hands.

7. In farther eviction of what has been advanced, relative to the initial letters in the exergues of certain Sidonian coins; it may not be improper to obſerve, that a medal in my ſmall collection exhibits the letter *Hheth*, immediately after the numerical inſcription in the exergue. This probably repreſents the word חצי, DIMIDIVM, HALF; as both that term and רבע, or רבוע, QVADRANS, QVARTER, are expreſſed at length on ſome

b Strab. *Geograph.* Lib. xvi. p. 1093. Amſtelædami, 1707.
c *Journ. des Sçav.* Aout 1760. p. 275. A Amſterdam, 1760.
d See Plate Fig.

part

medals of Croton, Corinth, and Syracuſe, (28) as well as upon an inedited Punic coin in my ſmall cabinet, was uſed for *Koph* by both the Phœnicians and Carthaginians in the earlier times. The form of the *Koph* likewiſe on a Punic medal, (29) that I formerly attempted to explain, was of a pretty high antiquity amongſt thoſe nations. Nor does (30) M. Pellerin merit any great attention, or regard, when he aſſigns that letter the power of *Aleph*; the character on the coin he refers to on this occaſion ſeeming not to point out *Aleph*, but *Koph*. Nor has he ſo much as offered to interpret the greateſt

of the [e] rarer Samaritan coins. Hence it ſhould ſeem inconteſtably clear, that the two Phœnician elements, ϒ Ш, prefixed to the ſame ſort of numerical characters, on ſimilar medals, muſt be viewed either in the ſame or a ſimilar light; which, in conjunction with the initial letters preſerved on the famous Samaritan medal of Bologna, formerly mentioned [f], muſt ſet the point here inſiſted on beyond diſpute.

8. From the preceding obſervations it ſeems manifeſtly to appear, not only that the alphabetic character immediately prefixed to the aforeſaid numerals is *Tzade*, but likewiſe that the coins on which theſe are impreſſed muſt be of a later date. Hence we may conclude, that the times in which thoſe pieces were ſtruck may, with a ſufficient degree of preciſion, be aſcertained.

(28) Joan. Bapt. Biancon. *De antiq. Lit. Hebræor. & Græcor. Libel.* p. 57, 63. Bononiæ, 1748.

(29) *De Num. quibuſd. Sam. & Phœn. Diſſert.* p. 86, 87. Oxonii, 1750.

(30) *Recueil de Medailles de Peuples et de Villes, &c.* Tom. III. p. 141, 142. A Paris, 1763.

[e] *Numiſm. Antiq. &c.* à Thom. Pembr. & Mont. Gomer. Com. *Collect.* P. 2. T. 85. Num. 7. Adr. Reland. *De Num. Veter. Hebræor.* Tab. Non. Num. 3. p. 202. Trajecti ad Rhenum, 1709.

[f] *Philoſoph. Tranſact.* Vol. L. P. ii. p. 792.

part

part of the legend, to which it belongs. 'Tis certain, that this character, as well as that on the Punic medal above-mentioned, bears so perfect a resemblance to the figure of *Koph* preserved by a coin of Cosyra, explained by the learned Sig. Abate Venuti, that it cannot well be taken for any other letter. As the objection therefore (31) offered by M. Pellerin to my notion of this character is destitute of every support, it must fall to the ground of course; and consequently no farther defence of that notion can be deemed requisite, or expected from,

S I R,

Your most obedient humble servant,

Ch. Ch. Oxon.
April 29, 1763.

John Swinton.

(31) *Recueil de Medailles de Peuples et de Villes*, &c. Tom. III. p. 141, 142, A Paris, 1763.

XXIII. *A Catalogue of the* Fifty Plants *from* Chelſea Garden, *preſented to the* Royal Society *by the worſhipful Company of Apothecaries, for the Year* 1763, *purſuant to the Direction of Sir* Hans Sloane, *Baronet, Med. Reg. et Soc. Reg. aliquando Præſes: By* John Wilmer, *M. D. clariſſ. Societatis Pharmaceut.* Lond. *Soc. Hort.* Chelſean. *Præfectus et Prælector Botanic.*

Read May 3, 1764.

2051 Achillæa foliis pinnatis ſupradecompoſitis laciniis linearibus diſtantibus. Flor. Leyd. Prod. 175.

2052 Achillæa foliis ſetaceis dentatis, denticulis ſubintegris ſubulatis reflexis. Linn. Sp. pl. 896.

2053 Achyranthes caule erecto, foliis ovatis, calycibus ſpicæ adpreſſis.

2054 Aconitum foliorum laciniis linearibus ſuperne latioribus linea exaratis. Hort. Cliff. 214.

2055 Æſchynomene leguminibus ſubæqualibus lævibus. Linn. Sp. Pl. 714.

2056 Angelica foliolis æqualibus ovatis, inciſo-ſerratis. Hort. Cliff. 97.

2057 Anthericum foliis carnoſis ſubulatis teretibus, ſcapo ſubramoſo. Hort. Upſal. 83.

2058 Aſperula foliis inferioribus ſenis, intermediis quaternis ſubulatis triquetris, floribus quadrifidis. Linn. Sp. Pl. 104.

2059 Aſtragalus (Chinenſis) cauleſcens procumbens, capitulis pedunculatis, leguminibus priſmaticis rectis triquetris, apice ſubulatis.

2060 Boerhavia caule diffuſo. Linn. Sp. Pl. 3.

2061 Caucalis umbella trifida, umbellulis triſpermis involucris triphyllis. Hort. Cliff. 91.

2062 Ceraſtium foliis lineari-lanceolatis tomentoſis, pedunculis ramoſis, capſulis globoſis.
Caryophyllus holoſteus tomentoſus anguſti-folius. C. B. P. 210.

2063 Chionanthus pedunculis trifidis trifloris. Linn. Sp. Pl. 8.

2064 Convolvulus foliis lanceolatis tomentoſis, floribus capitatis, calycibus hirſutis, caule erectiuſculo. Linn. Sp. Pl. 224.

2065 Cytiſus floribus capitatis, ramis decumbentibus. Prod. Leyd. 376.

2066 Dracocephalum floribus verticillatis, bracteis oblongis ovatis integerrimis, corollis majuſculis nutantibus. Hort. Upſal. 167.

2067 Echium corollis ſtamine longioribus. Linn. Sp. Pl. 140.
Echium ampliſſimo folio Luſitanicum. Tourn. Inſt. 135.

2068 Erigeron pedunculis alternis unifloris. Hort. Cliff. 407.

2069 Euphorbia inermis fruticoſa, foliis lanceolato-linearibus, floribus erectis, capſulis glabris.

2070 Euphorbia umbella quinquefida trifida dichotoma, involucellis ovatis, foliis lanceolatis, capſulis lanatis. Lin. Sp. Pl. 460.

2071 Ferula foliis pinnatifidis, pinnis linearibus planis trifidis. Hort. Cliff. 95.

2072 Ferula durior seu rigidis et brevissimis foliis. Bocc. Mus. p. 2. Tab. 76.

2073 Galium foliis octonis linearibus sulcatis, ramis floriferis brevibus. Hort. Cliff. 107.

2074 Gypsophylla foliis lanceolatis subtrinerviis rectis. Linn. Sp. Pl. 582.

2075 Hypericum floribus tetragynis, caule erecto herbaceo simplici, foliis integerrimis semiamplexicaulibus.

2076 Hypochæris calycibus æqualibus hispidis. Hort. Cliff. 385.

2077 Isatis foliis omnibus dentatis. Linn. Sp. Pl. 671.

2078 Lantana foliis oppositis subsessilibus, floribus racemosis. Linn. Syst. 1116.

2079 Mesembry-anthemum foliis triquetris acutis punctatis distinctis, calycinis foliolis ovato-cordatis. Hort. Cliff. 220.

2080 Nigella pistillis quinis, petalis integris, capsulis turbinatis. Linn. Sp. Pl. 753.

2081 Nigella pistillis denis corollam æquantibus. Hort. Upsal. 154.

2082 Nigella pistillis quinis, capsulis muricatis subrotundis, foliis subpilosis. Hort. Upsal. 154.

2083 Oenanthe umbellularum pedunculis marginalibus longioribus ramosis masculis. 62.

2084 Raphanus (Chinensis) siliquis torosis acuminatis foliis integris.

2085 Rudbeckia foliis lanceolato-ovatis hirsutis, radii petalis bifidis.

2086 Rumex floribus dioicis, foliis cordatis amplexicaulibus.

2087 Scabiosa corollulis quinquefidis, foliis dissectis, receptaculis florum subrotundis. Hort. Cliff. 31.

2088 Scabiosa corollulis quinquefidis radiantibus, foliis bipinnatis linearibus. Linn. Sp. Pl. 101.

2089 Scabiosa stellata folio laciniato minor sive maritima. C. B. P.

2090 Sisymbrium foliis pinnato-hastatis dentatis, siliquis erectis. Lin. Sp. Pl. 659.

2091 Scandix seminibus nitidis ovato-subulatis, umbellis sessilibus lateralibus. H. Cliff. 110.

2092 Scleranthus calycibus fructus patulis. Flor. Suec. 377.

2093 Solanum caule inermi herbaceo erecto, foliis ovatis integris glabris, umbellis axillaribus.

2094 Trifolium capitulis dimidiatis, foliis quinatis sessilibus, leguminibus polyspermis. Hort. Upsal. 223.

2095 Trifolium leguminibus racemosis nudis monospermis, caule erecto. H. Upsal. 223.

2096 Thalictrum caule folioso sulcato, foliis linearibus carnosis. Dalib. Paris. 162.

2097 Veratrum racemo composito, corollis patentissimis. Lin. Sp. 1044.

2098 Verbena tetrandra, spicis capitato-conicis, foliis serratis caule repente. Fl. Zeyl. 399.

2099 Veronica spicis terminalibus, foliis oppositis lineari-lanceolatis, acute serratis, caule erecto.

2100 Viburnum foliis serrulatis ovatis acuminatis glabris, petiolis marginatis. Linn. Sp. Pl. 384.

XXIV. *Observations on the Eclipse of the* Sun, April 1, 1764: *In a Letter to the Right Honourable* James *Earl of* Morton, *Pres. R. S. from the Rev.* Nathanael Bliss, *M. A.* Savilian *Professor of Mathematics at* Oxford, *and Astronomer Royal.*

My Lord,

Read May 10, 1764.

AS I had reason to believe, from a calculation made from the best lunar tables, that the north-west limit of the annular appearance, in the late great eclipse of the Sun, would pass but a few miles to the South-west of the Royal Observatory at Greenwich, I thought myself indispensably obliged, to leave Oxford, where my employment then called me; and to attend to an observation which might possibly be of some consequence. And I had at the same time an opportunity of paying my duty to their Royal Highnesses Prince WILLIAM HENRY, and Prince HENRY FREDERICK, who had signified their intention a few days before, of honouring the Royal Observatory with their presence on that occasion.

On the 1st of April, soon after eight in the morning, their Royal Highnesses arrived, and were pleased to signify to me, that it was their desire, notwithstanding their presence, that the observations might be made with all possible accuracy. Their Royal Highnesses were also pleased to permit his excellency the

the Neapolitan envoy extraordinary, the right honourable Lord Leigh, Dr. Morton, S. R. S. and ſeveral other gentlemen to be preſent.

Early in the morning the ſky ſeemed to promiſe to be favourable to us; but before the time when the eclipſe was expected to begin, it became ſo hazy that we almoſt deſpaired of making any obſervation at all. However Mr. Reeve, the aſſiſtant obſerver, was prepared to obſerve on the triangular leads, with a two foot reflecting teleſcope made by Mr. Short, and on March 31ſt, $21^h\ 5'\ 3''$ apparent time, he ſaw the firſt impreſſion made on the Sun's limb by the Moon; the ſky being got tolerably clear a few minutes before. Mr. John Bird, mathematical inſtrument maker in the Strand, with a two feet reflecting teleſcope made by himſelf, on the leads over the new chamber, did not ſee the beginning, by reaſon of a tremor, until ſix ſeconds later. I myſelf was endeavouring to obſerve it with an excellent refractor of 15 feet focal length in the great room: but, having at that time a watery defluxion on my eyes occaſioned by a cold, I was unfortunately obliged to wipe my eye perhaps at the very time of the contact: for at $21^h\ 5''\ 30''$ when I again applied my eye to the teleſcope and placed it on the object, the eclipſe was ſenſibly advanced. So that I apprehend the beginning as obſerved by Mr. Reeve to be very near the truth.

It had been before agreed that Mr. Reeve, to whoſe eye the reflecting teleſcope had been adjuſted when armed with Dollond's micrometer, ſhould obſerve the quantity of the lucid parts, as they decreaſed before the middle, and alſo as they increaſed after the

the middle was paſt; while Mr. Bird and myſelf, with the old micrometer applied to the 15 foot tube, ſhould meaſure the Moon's diameter as ſeen upon the Sun: But unfortunately, ſome time before the middle of the eclipſe, the hazineſs became ſo very thick that we loſt ſight of the Sun for many minutes. But as ſoon as the clouds began to diſperſe, Mr. Reeve obſerved the lucid parts as under, but did not aſcertain the time at either obſervation.

2′ 55″, 5—3′ 0″, 2—3′ 28″, 7—3′ 47″, 6 with ſeveral others that increaſed much faſter. By a mean of ſix obſervations made (as near the middle as the clouds would permit) both by Mr. Bird and myſelf, the extremes of which did not differ ſo much as 3″, the Moon's equatorial diameter was found to be 29′ 45½″ as ſeen on the Sun.

As the obſervations of the lucid parts were made as faſt as the numbers of the micrometer could be read off, and as the difference increaſed but ſlowly at firſt, we will ſuppoſe the two firſt obſervations to have been made not long after the time of the middle, and at the time of the firſt obſervation, the Sun was at leaſt eclipſed 10, 9 digits.

The Sun's horizontal diameter, as obſerved by Mr. Reeve, with the ſame micrometer, on the day before, and on the morning of the eclipſe, was 31′ 56½″, being a mean of ſix obſervations not ſenſibly differing.

About 11 o'clock the hazineſs became ſo thick that no further obſervations could be made, nor, at the time when the end was expected, could the Sun be ſeen.

At

At the obſervatory of the right honourable the Earl of Macclesfield at Shirburn Caſtle, the beginning of the eclipſe was obſerved at $21^h\ 0'\ 48''$ apparent time by one obſerver, and but one ſecond later by the other. And the end was obſerved at $23^h\ 56'\ 10''$; but this laſt obſervation is marked as very doubtful, the air being extremely hazy.

If your Lordſhip ſhould think the above obſervations worthy of the attention of the Royal Society, and will be pleaſed to communicate them to that learned body, it will very much oblige,

My Lord,

Your Lordſhip's and their moſt obedient humble ſervant,

Nathanael Bliſs.

XXV. *Ob-*

XXV. *Observations on the Eclipse of the* Sun, April 1, 1764: *In a Letter to the Right Honourable* James *Earl of* Morton, *Pres. R. S. from the Reverend* Thomas Hornsby, *M. A. and* Savilian *Professor of Astronomy at* Oxford.

My Lord,

Read May 10, 1764.

I Take the liberty to transmit to your Lordship the following observations of the great eclipse of the Sun on the 1st of April last, in hopes that your Lordship may think them not unworthy of the attention of that learned body, over which you preside.

On the morning of the 1st of April, the heavens were so uncommonly serene, that I could not but flatter myself with the hopes of a sky very favourable to observation. But about eight o'clock a haziness began to appear, and several clouds to arise from the South-west, which at small intervals deprived us of a sight of the Sun; these clouds however went off entirely to the North-east, and the Sun's limb appeared very nicely defined, and without the least undulation, through an excellent refracting telescope of 12 feet focus made by Mr. Bird, to which had been applied a system of eye-glasses similar to those used for reflectors, and the usual aperture of which had been contracted to $\overset{\text{Inch.}}{1,2}$. I continued to keep my eye very attentively fixed upon that part of the Sun's limb, where

where I expected the Moon would make the firſt impreſſion; and at $8^h\ 59'\ 33''$ apparent time, ſaw a very ſlight alteration on the Sun's limb, which I flatter myſelf may be conſidered as the time of the beginning of the eclipſe very accurately aſcertained; ſince a gentleman then preſent, with a reflecting telescope of 18 inches, ſaw that the eclipſe was begun about $6''$ later by the ſame clock.

As the eclipſe advanced, I propoſed to meaſure both chords and the quantity of the lucid parts with a reflecting teleſcope of 9 inches, armed with a micrometer executed by Mr. Dollond himſelf, and which was very obligingly left me for that purpoſe by my worthy collegue the reverend Mr. Bliſs. And accordingly at $9^h\ 37'\ 17''$ apparent time (as are all the times hereafter mentioned) I meaſured the chord of the part eclipſed and found it $= 25'\ 45'', 8$. At $9^h\ 50'\ 24''$ the unobſcured part of the Sun was $= 13'\ 17'', 7$; and at $10^h\ 3'\ 39'' = 8'\ 33'', 8$. At the time of which obſervation the colour of the ſky was remarkably changed.

At $10^h\ 19'\ 15''$ I determined, by the old micrometer applied to the 12 foot tube, the quantity of the lucid parts to be $3'\ 45''$: but, as I found ſuch meaſurements could not be taken with that inſtrument, without great difficulty, I immediately endeavoured to determine, by the other micrometer, the quantity of the Moon's horizontal diameter as ſeen upon the Sun, and found it, by a mean of three obſervations, hardly differing from each other, to be $29'\ 45'', 1$.

It was formerly a diſpute among the aſtronomers, whether the Moon's diameter did not appear leſs when viewed upon the Sun, than when ſeen upon a darker

darker ground. The obſervations of Mr. le Monnier in Scotland, in the year 1748, ſeemed to leave little room for doubt: and it is to be hoped that obſervations made with larger and better inſtruments than mine, and in places where the eclipſe was annular, or nearly ſo, will fully ſettle this point. At the time of the middle here, the Moon's centre was about 39 degrees high, and therefore the Moon appeared under a greater angle to the eye of the obſerver than if ſeen from the earth's center, by about 18 ſeconds. The true horizontal diameter from the above obſervations was therefore 29′ 27″; which is but 7″ leſs than according to the lateſt and beſt tables; which tables may perhaps give the Moon's diameter too large, becauſe conſtructed from obſervations made with refracting teleſcopes, through which the diameters, both of the Sun and Moon, muſt neceſſarily appear under an angle ſomewhat enlarged.

About the time of the middle of the eclipſe, I carefully attended to the diſtance of the cuſps: becauſe, from ſome calculations which had been laid before the public, the Northern limit of the annulus was to paſs within 4 or 5 miles of this place; and by other accounts I might have expected to have found myſelf conſiderably within the path of the annular penumbra. But as nearly as I could eſtimate by my eye, the diſtance of the cuſps was not leſs than $\frac{2}{7}$ of the whole circumference of the Sun. The quantity of the Sun's light was now very ſenſibly diminiſhed; a very diſtinct halo was ſeen at the diſtance of 12 or 14 degrees round the Sun; and we might have expected to have ſeen the planet Venus, had not the ſky been covered with a conſiderable hazineſs. As

 this

this hazineſs continued, I began to fear that I might be prevented from obſerving the end, and therefore meaſured, as carefully as unfavourable circumſtances would permit, the following chords.

	h	′	″		′	″
At	11	33	56	chord =	19	52, 5
	11	40	25	—— =	16	27, 9
	11	42	7	—— =	15	28, 5
	11	45	7	—— =	13	46, 0
	11	46	57	—— =	12	9, 4

But the ſky began to clear up; and at $11^h\ 58'\ 3''$ by the clock, or $11^h\ 54'\ 20\frac{1}{2}''$ apparent time, I obſerved the end, which I believe may be depended upon to 3 or 4 ſeconds.

About 20 minutes after the Sun had paſſed the meridian, I meaſured the Sun's horizontal diameter with Dollond's micrometer, and found it $= 32'\ 0'', 8$.

At ſeven o'clock in the morning, a Fahrenheit's thermometer, made by Mr. Bird and placed in the ſhade, ſtood at $42\frac{1}{2}$ degrees. At $7^h\ 32'$ I expoſed another thermometer of the ſame ſcale to the direct rays of the Sun. In five minutes the mercury roſe from 43 to 59. At $7^h\ 44'$ it ſtood at $67\frac{1}{2}$; and about twenty minutes before the eclipſe began this thermometer had riſen even to 84.

During

During the time of the eclipſe the following obſervations were made on each thermometer.

Apparent time. h ″	Thermometer in the ſhade.	Thermometer in the Sun.
At 9 11	53	62
9 30	53	
9 38	53$\frac{1}{4}$	70$\frac{1}{2}$
9 52	52$\frac{3}{4}$	69
10 14	51$\frac{1}{4}$	60$\frac{3}{4}$
10 32	49$\frac{1}{2}$	53$\frac{1}{2}$
10 44	49	53
10 50	49$\frac{1}{2}$	54$\frac{1}{2}$
11 20	51$\frac{1}{4}$	59
11 37	53$\frac{1}{4}$	63$\frac{1}{4}$
12 10	55$\frac{1}{2}$	66$\frac{1}{2}$

For ſeveral minutes before and after the middle of the eclipſe, the air was very ſenſibly colder.

I am, my Lord,

Your Lordſhip's moſt obedient
humble ſervant,

Oxford, April 30,
1764.

Thomas Hornſby.

XXVI. *Ob-*

XXVI. *Observations on the Eclipse of the* Sun, April 1, 1764: *By* Matthew Raper, *Esq; F. R. S.*

Read May 17, 1764.

THorley Hall. Lat. 51° 50′ 45″ N. Long. 38 s. east of Greenwich.

	h	m	s	
1764. Mar. 17. ☽ immerged into the true shadow at	10	41	—	apparent time.
emerged out of the same at	13	25	—	
Mar. 31. ☉ eclipse was begun above a minute at —	21	8	0	apparent time.
Ended April 1 at ———	0	1	45 or 48.	

Observed with an 8 foot refractor.

XXVII. *Table*

XXVII. *A Table of the Places of the Comet of* 1764 *discovered at the Observatory of the Marine at* Paris, *the 3d of* January, *about* 8 *o'clock in the Evening, in the Constellation of the* Dragon, *concluded from its Situation observed with regard to the Stars: By Monsieur* Charles Messier, *Astronomer at the Depot of the Plans of the Marine of* France, *at* Paris.

Read May 17, 1764.

1764	True time.	mean time	R. ascention observed.	Northern declination observ.	Longitude observed.	Northern latitude observed.
	h ′ ″	h ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″
Jan. 3.	9 24 33	9 29 9	236 29 16	58 32 58	♎ 11 37 16	72 53 48
	15 5 4	15 9 47	239 45 31	58 51 29	14 49 7	74 22 13
	18 48 55	18 53 42	241 56 1	59 2 54	17 9 14	75 19 50
10	16 10 39	16 18 28	305 57 17	44 23 10	♒ 29 0 38	60 41 41
	18 33 53	18 41 44	306 22 2	44 6 14	29 15 54	60 18 20
11	6 43 27	6 51 31	308 14 21	42 45 45	♓ 0 21 59	58 28 49
	7 30 54	7 38 59	308 20 17	42 42 35	0 26 19	58 23 58
	18 28 33	18 36 48	309 50 42	41 22 54	1 7 31	56 42 7
14	17 7 58	17 17 22	316 59 44	34 37 35	4 23 15	48 8 16
	18 32 53	18 42 18	317 5 44	34 30 37	4 25 22	47 59 50
15	5 43 14	5 52 48	317 53 29	33 39 23	5 8 47	47 6 48
	7 13 15	7 22 51	317 59 44	33 31 50	5 20 11	47 1 47
16	6 35 19	6 45 15	319 28 52	31 51 34	5 22 51	44 44 33
	7 26 11	7 35 58	319 32 37	31 48 41	5 24 58	44 40 44
18	5 40 32	5 51 8	321 52 36	28 45 54	6 9 39	41 4 3
	7 4 45	7 15 22	321 56 21	28 40 52	6 10 49	40 58 8
19	7 42 8	7 53 4	322 57 8	27 17 25	6 29 47	39 19 57
20	6 50 20	7 1 34	323 46 38	26 5 33	6 43 22	37 56 17
22	5 49 50	6 1 38	325 9 52	23 49 1	7 0 56	35 20 54
	6 11 16	6 23 4	325 10 22	23 47 57	7 0 57	35 19 45
29	5 50 59	6 4 23	328 9 59	17 28 48	7 7 46	28 25 8
30	6 2 52	6 16 28	328 23 44	16 42 10	7 1 12	27 36 59
Feb. 4	5 57 53	6 12 8	329 17 41	13 11 38	6 26 10	24 2 24
7	6 30 23	6 44 54	329 33 51	11 6 15	5 50 20	22 0 9
8	6 16 20	6 30 54	329 39 35	10 26 47	5 41 16	21 21 25
11	6 14 49	6 29 28	329 41 27	9 2 46	5 10 5	20 2 34

I have the honour to send you likewise the elements of the theory of this comet, which monsieur Pingré has deduced from my first observations, as follows.

	s	°	′	″
The ascending node ☊ — —	3	29	20	6
Inclination — —		53	54	19
Place of perihelium — —		16	11	48

Logarithm of the distance of the perihelium 9.751415.

Passage by the perihelium 12 February at $10^h\ 29'$ mean time in the meridian of Paris. The motion retrograde.

XXVIII. *A Supplement to Monſ.* Pingré's *Memoir on the Parallax of the* Sun: *In a Letter from him to the Royal Society, Tranſlated by* M. Maty, *M. D. F. R. S.*

Gentlemen,

Read May 17, 1764.

I Had the honour to ſend you my memoir on the Parallax of the Sun as deduced from the obſervations of Venus; give me leave to add to it ſome remarks which I have ſince made, and communicated to our academy. The learned Societies of Europe, amongſt which yours holds a moſt diſtinguiſhed rank, will be the judges of the ſucceſs of our expeditions. A definitive deciſion will probably not be formed till after the obſervation of the tranſit of 1769; in expectation of further accounts, which may tend towards this deciſion, there is at leaſt one which I now ſubmit to your examination.

Supplement to my Memoir on the Parallax of the Sun.

I impatiently expected the firſt volume of the Philoſophical Tranſactions of the year 1761, where I hoped to find ſome obſervations that might determine which of the obſervations, *viz.* that of Meſſ. Maſon and Dixon made at the Cape of Good-Hope, or that which was made at the iſland Rodriguez by Mr. Thuillier and myſelf, deſerved the preference. The firſt reduces the Sun's Parallax to $8'' \frac{1}{2}$ at moſt,

whereas

whereas the latter increases it to near $10''\frac{1}{2}$: the difference is too considerable not to deserve an enquiry into its causes. The expected volume is at last come to hand; and my first care has been to examine with the most scrupulous attention the observation made at the Cape. I owe this testimony to truth, that this observation, as well as all the others of Mess. Mason and Dixon, appears to me to have been made with great judgment. An exception might however be made as to the extensive description of an appulse of σ Sagittarii to the Moon's southern limb, which is found page 389. This certainly was more than an appulse, and the star was really immerged at the Cape.

In this same volume, I found two observations, which would be decisive, if time and other circumstances had permitted them to be made with sufficient accuracy. I have very carefully calculated them both. Mr. Maskeline observed at the island of St. Helena, situated at $15^{d}\ 55'$ South latitude, and according to Dr. Halley at $33'\ 17''$ of time West of the Observatory at Paris; but this determination of the longitude does not seem sufficiently exact. I have compared many observations of Jupiter's Satellite's immersions and emersions made at the island of St. Helena by Mr. Maskeline with the corresponding ones made at Paris at the Marine Observatory by Mr. Messier, and have only found $31'\ 56''$ for the difference of longitude between the two places; and as the Marine Observatory is $2''$ East of the Royal Observatory, I think I may conclude that the place where Mr. Maskeline observed is only at $31'\ 54''$ West of the Royal Observatory.

The clouds greatly obſtructed Mr. Maſkeline's obſervations; he could make but one that was uſeful. At $7^h\ 31'\ 07''$ in the morning, apparent time, the bodies both of the Sun and of Venus being perfectly well defined, the diſtance of the neareſt limbs was, by means of an object-glaſs Micrometer adapted to a reflecting teleſcope according to Mr. Dollond's invention, found to be $1'\ 44''\frac{3}{4}$. I have computed that by allowing $10''$ for the horizontal parallax of the Sun, from that phaſis to the internal contact of the limbs, there muſt have paſſed $34'\ 52''$ at St. Helena; I have even aſſured myſelf, by ſome other calculations, that, by an alteration of one or two ſeconds in the parallax, and of ſeveral minutes of time in the longitude of St. Helena, the interval which I have juſt now determined would ſuffer an increaſe or diminution but of very few ſeconds. The internal contact of the limbs muſt therefore have happened at St. Helena at $8^h\ 05'\ 59''$ in the morning. One can't ſuppoſe it to have happened later, becauſe $17'$ after, or at about $8^h 23'$, the ſkies having cleared again, nothing more could be ſeen, and the external contact was over. This obſervation being compared with that of Tobolſk, would give $11''$ for the horizontal parallax, which is a little too much. Mr. Maſkeline obſerves, that tho' Venus's limb and the Sun's appeared as defined as could be deſired, yet when the artificial internal contact of Venus's limb with the Sun's was made by means of the object-glaſs micrometer, Venus's limb dilated and contracted itſelf alternately, getting and loſing a ſmall ſpace within the Sun's limb. He adds, that he endeavoured to take it in the middle of this vibration, but dares not affirm that he exactly

actly did fo. Let the diftance determined by Mr. Mafkeline be only diminifhed by 2″, and his obfervation will perfectly agree with mine; but in order to make it tally with that of the Cape, it would be neceffary to diminifh this diftance by 10 or 11″, and it is not very likely that Mr. Mafkeline fhould have committed fuch an error. His obfervation might likewife be brought to coincide with mine, by making a diminution of 40 or 45″ in the Weftern longitude of St. Helena, as I juft now fettled it; whereas it would be neceffary to take feveral minutes from that longitude in order to make the obfervation agree with that of the Cape, which does not feem poffible. Hence, though I don't take Mr. Mafkeline's obfervation to be in itfelf abfolutely decifive, yet I am perfwaded that it adds great weight to the exactnefs of mine.

Fort St. George at Madrafs is, according to Mr. Hirft, $13^d\, 8'$ North latitude and $3'\, 4''$ of time Eaftward of Pondicherry, and confequently at leaft $5^h 12' 54''$ Eaftward of our Obfervatory's meridian. Mr. Hirft's obfervation is related in the Philofophical Tranfactions; the interval obferved by him between the two internal contacts was $5^h 51' 43''$, greater by $2' 49''\frac{1}{4}$ than that which was obferved at Tobolfk. This would give $9''\, 56$ for the Sun's horizontal parallax, a quantity which is about a medium between the Cape obfervation and mine. The comparifon between the time of obferving the contacts, and that of the correfponding obfervations made in other places, gives conclufions fo vaftly different, that I dare not relate them here. Mr. Hirft gives a fufficient account of his inftruments, but he does not fay whether his

 pendulum

pendulum or clock was one with weights. He ſays he regulated his clock by equal altitudes, and then by meridional paſſages of Spica Virginis, and of the Sun; but why did he leave off the method of the equal altitudes which he made uſe of at firſt, and in what manner did he obſerve theſe altitudes to determine the paſſage either of a fixed ſtar or of the Sun over the meridian? It does not appear that he had a quadrant or tranſit inſtrument. I am ſorry that this uncertainty about the means employed by Mr. Hirſt to determine the time of the phaſes puts it out of my power to make uſe of an obſervation, which might otherwiſe have been extremely uſeful, had the aſtronomer been equally well provided with inſtruments as he appears to have knowledge and zeal. It is to be obſerved that by increaſing or diminiſhing by 10″ the duration obſerved at Madraſs, the queſtion of the parallax will be decided conformably either to the obſervation of Rodriguez or that of the Cape.

In the ſame volume of the Tranſactions, are ſome obſervations of the ſame tranſit made at Abo and at Hernoſand; the total duration was obſerved in both places; it may have been lengthened ſomewhat beyond its limits; but theſe obſervations agree at leaſt in this point with all the others that were made in the North, viz. that being compared with the Tobolſk obſervation, with regard to the duration of the tranſit, they give above 10″ for the horizontal parallax of the Sun.

I have likewiſe lately had the communication of Mr. Rumowſki's obſervation made at Selenginſk in Siberia. I ſhall not expatiate upon the particulars here,

 ſuppoſing

ſuppoſing that you Gentlemen have received it. The latitude of Selenginſk is $51^d 6' 6''$. I have ſettled the longitude to be $6^h 57' 50''$ from the Paris meridian. Some immerſions of the firſt and ſecond ſatellite of Jupiter have given me $6^h 57' 15''$, $6^h 57' 20''$, and $6^h 58' 31''$, by comparing the obſervations of Mr. Rumowſki with the tables corrected upon the obſervations made at Paris and at the Cape, and eſtimating, as well as I was able, the different effects of the reflectors and teleſcopes. The 15th of July, Mr. Le Monnier obſerved the meridional tranſit of ϕ Sagittarii at $18^h 25' 38'' \frac{2}{3}$ of his clock, being at $76° 00' 45''$ from the zenith. The preceding limb of the Moon paſſed at $18^h 38' 28'' \frac{1}{2}$, or at $11^h 01' 40''$ apparent time; when the center paſſed, the diſtance of the upper limb was at $75^d 52' 00''$ from the zenith, and the lower at $76^d 22' 25''$; the threads, which are $5''$ thick, being entirely upon the Moon. σ Sagittarii had paſſed at $18^h 35' 22'' \frac{1}{2}$, the diſtance from the zenith being $75^d 22' 20''$. I have obſerved at Rodriguez the immerſion of σ Sagittarii at $14^h 01' 28'' \frac{1}{2}$ apparent time, and Mr. Rumowſki obſerved at Selenginſk the immerſion of ϕ at $11^h 24' 51''$ apparent time. Upon comparing all theſe things together, I find the longitude of Selenginſk to be $6^h 57' 21'' \frac{1}{2}$ Eaſt from Paris. But this goes upon the ſuppoſition that the error of the tables has been quite conſtant during near 7 hours, which cannot be warranted. Laſtly, the obſervation of the eclipſe of the Sun made at Selenginſk June 3, 1761, compared with the ſame obſervation made at Tobolſk and at Cajanebourg, determines the longitude of Selenginſk $5^h 16' 41'' \frac{1}{2}$ Eaſt from Cajanebourg, and at $2^h 34' 30''$ Eaſt from Tobolſk, conſequently $6^h 58' 22''$ Eaſt

pect it. My clock went too slow that day at noon by 2'2", so that I ought to have added 1'1" to the time of the clock, whereas I substracted as much from it. This is the reason of the difference in the times. The small variation in the distances of the limbs was owing to a stricter verification of the parts of my micrometer.

I am, with the most respectful esteem,

Gentlemen,

Paris, Feb. 14, 1764.

your most obedient,

humble servant,

Pingré.

XXIX. *An Account of the Transit of Venus: In a Letter to* Charles Morton, *M. D. Secret. R. S. from* Christian Mayer, *S. J. Translated from the* Latin *by* James Parsons, *M. D.*

Read Feb. 4, 1762.

I Return you many thanks for the great trouble you have taken in procuring Mr. Dollond's telescope for me; which, happening to arrive very opportunely the day before the observation, gave great pleasure to our Serene Elector: a very happy invention which England alone was capable of producing! but at it's coming to my hands I had no small concern, for fear all our apparatus should be rendered vain, as it was constant rainy weather.

A square mount of solid stone which had been made into an arch, in the Electoral garden at Schwesinga by his Highness's order, afforded us a basis; in the middle of which another mount of like form was raised five feet high, which supported the astronomical quadrant: both were covered with a moveable covering, the building being carried round them.

Two other small buildings of the same construction stood near this; in one of which Mr. Dollond's telescope was placed, and in the other the clock; having so easy a communication with one another, that a glance of the eye commanded them all.

The astronomical quadrant, which was $2\frac{1}{2}$ feet radius Paris measure, was made in the year 1758, at Paris by M. Carinivet mechanical operator to the

Royal Academy, and has an Engliſh micrometer; having a moveable wheel, and divided into minutes; and by means of a ſcrew to the index of the quadrant, together with the diviſion of the nonnius plate faſtened to it, was ſo applied, that during the whole time of the obſervation, while the wire of the plummet conſtantly glided upon the ſame point of the limb, it might be moved in the limb by a vertical motion in either direction by the alidad alone. This moſt excellent invention of your's I firſt brought into Germany, to the beſt of my knowlege, after I had ſeen it's power at Paris in the hands of the ingenious M. Le Monier.

Beſides the quadrant, Dollond's teleſcope, and ſeveral other aſtronomical tubes of 6, 8, 13, and 22 feet; we had a Newtonian teleſcope of 4 feet Auſtrian meaſure, with an eye glaſs of $\frac{1}{3}$ of an inch.

We had a clock made by M. le Paute, a Pariſian, very well defended from the rays of the Sun and from the wind, which I accommodated to this buſineſs for a month before, in many celeſtial obſervations with as much accuracy as I could, and with more ſucceſs than we could well expect.

	True time.		
	h	′	″
The interior contact of the weſtern limb of Venus, with the weſtern limb of the Sun, obſerved with Dollond's teleſcope — —	20	53	8
The moment of the egreſs, wherein the ſame limb of the Sun after the interior contact firſt appeared corniculated, moſt accurately obſerved with the ſame teleſcope, was — —	20	53	35
Whence I conclude, that the interior contact happened — —	20	53	$33\frac{1}{2}$

As to the inſtant of the exterior contact, I ſend only two obſervations made by me with certainty, becauſe of the intervening clouds: the firſt ſhews the time wherein I diſtinctly ſaw through the clouds the certain emerſion of Venus, $\frac{1}{12}$ part of the diameter of Venus, as nearly as I could judge, excavating the limb of the Sun; the other wherein, from the unlucky clouds, I could no more obſerve the leaſt veſtige either of the emerſion, or exterior contact, or of Venus.

	True time.		
	h	′	″
The firſt outer contact — —	21	9	4
The other time of the certain emerſion —	24	17	27

The time which I uſe, and to which I am ſtill attentive, was obtained by a great number of correſponding altitudes of the Sun, both before and after the day of the tranſit. But in order to the rendering my calculation perfect one thing is to be deſired, that the longitude and latitude of the obſervatory at Schwezinga might be preciſely determined, if poſſible, in the ſpace of a few months.

The Serene Elector, although he was not well the day before, yet from his great love for aſtronomy was not only preſent himſelf, which other kings and princes uſually are; but being of an excellent and ready genius, he inſtituted ſeveral obſervations with the aſtronomical quadrant two hours before the egreſs, having marked the appulſes of the four limbs of the Sun and Venus at each immovable wire, in

order to find out the difference of the right afcenfion and declinations; the weather being ferene from about half an hour after five to half after eight in the morning. Which pofitions, good Sir, if they may be acceptable, I will immediately fend you; and likewife fome obfervations upon a total eclipfe of the Moon at which his Serene Highnefs and his whole court were prefent, together with two very refpectable perfons of the court, the illuftrious Meff. Holrinhaufen and de Stanger, both well fkilled in aftronomical matters, who were appointed to be my affiftants in all thefe obfervations.

Hence you will eafily perceive, that I am attached to the moft earneft endeavour of pleafing fo great a Prince; who, taking into his protection all kinds of learning, was the firft that introduced aftronomy into his country, the name thereof being unknown here for fo many ages.

It will be a great fatisfaction to me if this our obfervation may obtain a place among the immortal monuments of your Society; that the future clafs of aftronomers may retain a grateful remembrance of the moft ferene Charles Theodore Elector Palatine: and while I am, learned Sir, in expectation of this favour from the Royal Society, I humbly commend myfelf to your regard, being,

Your moft humble fervant,

Dated at Schwezinga,
23 June, 1761.

Chriftianus Mayer, S. J.

XXX. *Ob-*

XXX. *Observationes Astronomicæ* Christiani Mayer, *S. J.*

Illustrissimo ac celeberrimo Viro ac Domino Carolo de Morton, *Academiæ Regiæ* Londinensis *a secretis, S. P. dicit* Christianus Mayer, *S. J. S. E. P. Astronomus.*

Read May 24, 1764.

OBservationem meam eclipsis Lunaris quam hic tibi sisto, quamque circa 20mam Martii Parisios jam miseram, ex responso clarissimi viri de la Lande, die 3 Aprilis ad me dato, audio Parisiis erroris 10 minutorum suspectam haberi, quibus ejus durationem justo minorem collegissem: cum enim ex initio et fine certo hujus meæ observationis inveniam quantitatem durationis $2^h\ 46'\ 51''$, eam celeberrimus ephemeridum Parisinarum conditor de la Landius putat esse oportere $2^h\ 56'$. Non potuit, ut scribit laudatus astronomus, eclipsis hæc diei 17 Martii, nec illa 1 Aprilis, ob nubes Lutetiæ observari: potuit fortasse Londini, et Grenovici; quod si ita, omnem facile controversiam diremptam video. Si enim Londini initium hujus eclypsis acciderit $10^h\ 31'$ circiter, certum est idem in meridiano Parisino 9 minutis orientaliore accidisse $10^h\ 40'\ 40''$, ut habent ephemerides Parisinæ; sin minus, si Londini initium hujus eclipsis observatum fuerit $10^h\ 39'\ 5''$, perspicuum est errorem 10 minutorum cadere in ephemerides, et nequaquam in observationem meam, quæ hac ratione statutæ meridianorum differentiæ ubique terrarum optime congruet: siquidem ex aliis primi satellitis

Jovis

Jovis inſtitutis eclipſim comparationibus Schwetzingam invenio 31′ in tempore Viennâ occidentaliorem.

Quare cum rumor hic aulam noſtram pervaſerit, me in obſervatione eclipſeos hujus lunaris 10 minutis aberraſſe, ad te, vir celeberrime, mihi confugiendum fuit, quem hiſce demiſſe oro, ut celeri reſponſo, quid actum ſit Londini, quid Grenovici, mihi ſignificare digneris.

Rem quoque mihi feceris gratiſſimam, ſi ad me perſcripſeris, quæ ſint meliores magiſque uſitatæ in Anglia cœleſtes ephemerides pro annis futuris, qui tuis me favoribus toto animo demiſſiſſimè commendo.

Illuſtriſſimæ tuæ dignitatis

Servus infimus

Heidelbergæ,
17 Aprilis, 1764.

Chriſtianus Mayer, S. J.

Sereniſſ. Elector. Palatin. Aſtron.

Immerſiones

Immersiones et emersiones macularum ☽ in eclipsi Lunæ diei 17 Martii, Schwesingæ in Palatinatu prope Heidelbergam, observatæ à P. Christiano Mayer, è S. J. tubo Dollondi 10 pedum.

	Tempus verum.		
	h	′	″
Penumbra cœpit ——	11	2	24
Penumbra densior —— ——		8	11
Initium dubium —— ——		13	8
Eclipsis videtur cœpisse ——		14	9
Mare humorum stringitur ab umbra ——		25	22
Mare humorum medium in umbra		27	24
Tycho incipit immergi ——		29	58
Tycho totus in umbra —— ——		30	21
Grimaldus proximus umbræ ——		31	31
Grimaldus et Schicardus in umbra —		34	15
Mare nubium ingreditur umbram ——		39	30
Gassendus ad limitem umbræ ——		41	20
Insula sinus medii in umbra ——		46	35
Landspergius in umbra ——		47	33
Umbra Keplerum stringit ——		48	8
Keplerus ultra dimidium in umbra		51	3
Keplerus totus in umbra ——		52	4
Copernicus stringitur ab umbra		59	15
Copernicus totus in umbra ——	12	2	26
Mare fœcundatis fere medium in umbra		6	40
Mare tranquillitatis incipit ——		8	43
Palus somnii incipit esse in umbra ——		10	16
Mare tranquillitats ultra med. in umbra		13	55
Taruntius totus in umbra ——		18	14
Plinius et Menelaus ad umbram ——		20	18
Mare serenitatis stringitur ab umbra ——		22	23

Proclus

	Tempus verum.		
	h	′	″
Proclus ad umbram	12	27	8
Mare Criſium ultra med. in umbra		32	44
Mare Criſium ſeu Caſpium tot. in umbra		38	1
Mare ſerenitatis med. in umbra		41	28
Copernicus incipit egredi		58	35
Copernicus dimidius ex umbra		59	57
Limbus umbræ ſtringit Copernicum	13	1	57
Menelaus emerſit		8	21
Manilius videtur egredi		11	35
Mare humorum med. ex umbra		13	45
Manilius videtur egreſſus		14	51
Mare ſerenitatis extra umbram		17	2
Sinus æſtuum et mare humorum tot. ex umbra		20	25
Mare nubium medium ex umbra		23	41
Mare Caſpium egreditur		25	15
Umbra paludem ſomnii deſerit		30	38
Tycho emergit cum dimidio mari Criſio		32	30
Tycho certo emerſit		35	49
Taruntius videtur egreſſus		41	27
Mare tranquillitatis ultra med. emerſit		46	36
Langrenus emergit cum Dionyſio		47	25
Mare fœcunditatis fere tot. emerſit		52	18
Langrenus certo ex umbra		55	22
Videtur umbra mixta luci		59	11
Finis eclipſis dubius	14	0	11
Finis certus		1	0
Penumbra reſidua		3	10
Finis penumbræ		4	0

Præter has maculas, à me dimenſæ ſunt phaſes 20: et tempore maximæ obſcurationis $12^h\ 37'$, inventa obſcuratio

ſcuratio maxima $2388\frac{1}{2}$ partium micrometri (quod tubo ſeparato 6 pedum accommodaveram) quarum diameter Lunæ inventa eſt $3313 = 33' 49''$; unde meo quidem calculo obſcuratio maxima deducitur 8 digit. 39 minut.

Obſervatio eclipſis Solaris facta Schwetzingæ in obſervatorio Electorali anno 1764, die civil. N. S. 1ma Aprilis tubo dioptrico 6 pedum micrometro armato, cœlo ſereno, barometro 27 pollic. 2 lin. præſentibus illuſtr. et excell. D. D. O Dunne Miniſtro Galliæ, et Comite de Riancour Legato Saxoniæ.

IMMERSIONES.

Tempus verum. h	′	″		Partes centeſ. diſci ☉ obſcur. menſurantes ſagittam ad chordam duo cornua ſecantem normalis.	Partes centeſimæ diſci ☉ lucidi.	
21	40	41	videtur eclipſis coepiſſe.			
	43	2	duplex cornu ſenſibile.			
	48	4		113		
	51	53		157		
	56	9		251		
22	4	33		418		
	6	52		449		
	9	43		508		
	11	22		536		
	14	29		611		
	19	46		$725\frac{3}{4}$		
	23	43			1751	
	25	13			1709	
	33	16			1468	
	38	26			1273	
	42	40			1162	
	48	6			$987\frac{1}{4}$	
	52	40			861	
	55	14			762	
	58	7			658	
23	1	33			574	
	3	54			513	
	8	3			416	
	10	56			409	10 dig. $25\frac{1}{2}$ min.

E M E R S I O N E S.

Tempus verum. h	′	″	Partes centeſ. diſci ☉ obſcur. menſurantes ſagittam ad chordam duo cornua ſecantem normalis.	Partes centeſimæ diſci ☉ lucidi.
23	13	40		414
	14	0		421
	18	59		540
	21	49		621
	24	36		705
	27	21		777¼
	31	33		890
	33	53		986
	37	29		1091¼
	40	45		1209
	45	58		1367
	50	58		1526
	53	43		1606¾
h	57	48		1729¼
0	2	29		1860¾
	9	4		2062
	14	20	494	
	17	38	434	
	25	17	311	
	27	20	259	
	33	42	152	
	37	16	110	
	43	0	finis certus eclipſis.	

Diameter ſolis in partibus centeſimis micrometri probe verificati $3117 = 32' \, 4'', 8$.

Nondum licuit phaſes has omnes acuratius examinare; quarum priores immerſionis, in maxima hominum frequentia, minus exactæ ſunt.

XXXI. *Obſervations on the Eclipſe of the* Sun *at* Chatham, April 1, 1764, *by Mr.* Mungo Murray: *Communicated to* Joſeph Salvadore, *Eſq;* *F. R. S. in a Letter from Dr.* John Bevis.

Sir,

Read May 24, 1764.

I Fancy I can now ſatisfy your curioſity as to a place in the northern limit of the path of the Moon's ſhadow, in the eclipſe we obſerved at your houſe; that is, where the lower limbs of the Sun and Moon coincided, by the following abſtract from a letter I received from my friend Mr. Mungo Murray of Chatham, a good mathematician, and author of an excellent work on ſhip-building.

"I am infinitely obliged to you for your kind
" preſent of the teleſcope glaſſes. I got them moſt
" curiouſly mounted, and, as you ſaid, they make
" a 12 foot teleſcope, which takes in the whole Sun
" nearly. I ſet my watch by a very good vertical
" ſun-dial, preciſely at 9 o'clock, and at 8 minutes
" after I perceived the Moon juſt enter on the Sun.
" About half an hour after 10, the eclipſe was barely
" *annular*, the light of the Sun below the Moon be-
" ing but juſt viſible, and leſs than a hair in the tele-
" ſcope. At 55 minutes paſt 11 the eclipſe ended,
" and left the Sun quite round."

By this, Sir, I think you may ſafely conclude, that Chatham was not much more than a mile (perhaps leſs) ſouth-eaſt of the limit; which therefore paſſed over Rocheſter-bridge, or very near it.

Your moſt obedient humble ſervant,

John Bevis.

XXXII. *Observations and Experiments on different Extracts of* Hemlock: *By* Michael Morris, *M. D. F. R. S.*

Read May 24, 1764.

DR. Wade, an eminent phyſician at Liſbon, having lately communicated to the London Medical Society, a number of caſſes, in which the extract of Hemlock prepared at Coimbra in Portugal, had been given with extraordinary ſucceſs, and having ſent me at the ſame time ſpecimens of the ſucceſsful extract, and alſo of the extracts of Hemlock prepared at Liſbon and by Dr. Storck's Apothecary at Vienna, which two laſt-mentioned extracts he had preſcribed for the ſpace of three years, in various diſorders, to little or no effect; I thought an experimental inquiry into the component parts of theſe extracts and that uſed in London might be attended with ſome uſeful or curious conſequences; more eſpecially as this medicine was near loſing its credit intirely, from its little ſucceſs *here* in thoſe diſorders in which it had been moſt ſtrongly recommended by Dr. Storck. I think it not unneceſſary to premiſe farther, that the extract prepared at Coimbra is not ſo moiſt as the other extracts, and that it has been given for a conſiderable time at the doſe of a drachm and a half twice a day without producing the leaſt diſagreeable ſymptom.

EXPERIMENT I.

$2\frac{1}{4}$ grains of the extract of Hemlock prepared at Coimbra, digeſted with an ounce of highly rectifyed ſpirit of wine for 36 hours in a warm room, gave a browniſh yellow tincture; the clear liquor being poured off, a freſh quantity of ſpirit was added as before, and expoſed to digeſtion for the ſame ſpace of time; the ſecond tincture was conſiderably leſs coloured; this, added to the former tincture, was filtered, and expoſed to the air in a warm room until the ſpirit was intirely evaporated; the dry reſiduum weighed five grains; on expoſing it to the air it became ſofter, and even moiſt at the ſurface.

On pouring ſome water on the reſiduum now moiſt, it was ſoon tinged of a browniſh yellow, which being poured off, and a freſh quantity added at different times, until an ounce and a half of water had been uſed, there remained ſome blackiſh matter not ſoluble in water, which when dry weighed one grain, did not attract the moiſture of the air, melted and burned with a bright flame when expoſed to the fire, was ſoluble in ſpirit of wine, and had every characteriſtic of a rezin.

The tinged water, which had been ſeparated from this rezin and filtered, was evaporated ſlowly, until a brown dry matter remained weighing three grains, which in a few hours attracted the moiſture of the air, and relented into a dark brown thick liquour, of a ſaline taſte, and the ſmell peculiar to the extract of Hemlock. One drop of this liquor, diluted with a little water, deſtroyed the colour of ten times the

quantity

quantity of ſyrop of violets, without giving it the leaſt red tint; reducing it, on adding ſome drops of oleum tartari per deliquium, it ſuffered no remarkable change. Spirit of ſalt did not occaſion any alteration in it. But with oil of vitriol there was a ſtrong efferveſcence, without any ſenſible fume.

It appears from the above experiments, that the Coimbra extract of Hemlock contains one fifth ſoluble in ſpirit of wine, $\frac{3}{5}$ths of which conſiſt of an oily eſſential ſalt, the remainder being a reſin.

EXPERIMENT II.

The extract of Hemlock from Vienna was ſofter than that from Coimbra; on breaking it, there appeared ſmall whitiſh ſtreaks on each ſurface. 24 grains of it, treated as in the former experiments with ſpirit of wine, gave a fine deep green tincture, which on evaporation gave a reſiduum of a dark green towards the edges of the cup and a dark brown towards the middle; the whole reſiduum when dry weighed two grains and $\frac{1}{4}$; on leaving it expoſed to the air, the brown matter attracted moiſture from it and relented into a thick brown liquour; on adding water to it, as in the experiments on the Coimbra extract, the ſolution was of a light green colour; on evaporation it gave $\frac{3}{4}$ths of a grain of dark brown reſiduum, which ran per deliquium into a brown liquour, differing only in colour from that obtained by a ſimilar proceſs from the Coimbra extract. The undiſſolved reſinous matter weighed $\frac{1}{2}$ grain, was of a greeniſh colour, but in other reſpects like the reſin of the Coimbra extract. It appears from the green

tincture

tincture communicated both to water and rectifyed spirit by the Vienna extract, that the Hemlock had been gathered too soon, and before the plant was in vigour.

E X P. III.

The spirituous tincture of the Lisbon extract was not so green, nor was the green so durable, as that of the Vienna extract: the phænomena, in consequence of the other experiments, did not differ materially from those of the Vienna extract.

E X P. IV.

The spirituous tincture of the extract of Hemlock prepared at the Apothecary's-Hall, was like in colour to that of Coimbra, but the residuum did not differ considerably from that of Vienna and Lisbon. This extract has been used with some success at the Westminster-Hospital.

E X P. V.

The spirituous tincture of the powdered leaves of Hemlock was like in colour to the last; the residuum differed materially from that of the former extracts only in its rezin's being considerably more fluid.

These experiments shew that the extract of Hemlock prepared at Coimbra contains a far greater quantity of an essential oily salt and rezin, than the other extracts. As the oils, salts and rezins are the most act-

tive parts of vegetables, may not the well-attested salutary effects of the Coimbra extract be owing to its greater quantity of these active principles, particularly if we consider the large dose it has been prescribed in? As these active oily salts and rezins are soluble in spirit of wine, we have the means of obtaining them from the extract of our own Hemlock in sufficient quantities for use, and without fatiguing the stomach with the nauseous inactive parts of the extract.

But as experience alone can shew whether the virtues of the Hemlock reside in the whole extract, or in the saponaceous parts soluble in spirit of wine; I shall content myself with proposing these few hints, until experiments shall enable me to lay the other consequences of these assays with proper weight before the Society.

XXXIII. *An*

XXXIII. *Eſſay on the Uſe of the Ganglions of the Nerves: By* James Johnſtone, *M. D. Communicated by the Right Rev.* Charles *Lord Biſhop of* Carliſle, *and F. R. S.*

Read May 31, 1764.

THE Ganglions of the incercoſtal nerves, firſt diſcovered by Fallopius, are oblong and very hard bodies; the uſes of which have not been ſatisfactorily aſcertained by any one. Few anatomiſts have indeed entered deep into the ſubject, except the learned J. M. Lanciſi, who imagined them muſcles ſui generis, and, like other muſcles, capable of contractions; by which he thought the nervous ſpirits were acclerated and impelled with ſuch additional forces, as are by him ſuppoſed neceſſary to the production of motions in muſcles ſubject to the will: And in order to give an idea of the ſtructure of all other Ganglions, he particularly deſcribes and delineates that of the firſt cervical Ganglion (*a*).

This theory has the misfortune to be erroneous in its foundation. For Haller and other ſucceeding anatomiſts have not been able to diſcover this muſcular apparatus in the firſt cervical Ganglion (*b*). The coats of Ganglions I have found with the appearance and firmneſs of ligaments; but incapable of ſuch extenſion or retraction, as elaſtic muſcular fibres always allow of.

(*a*) See Lanciſi's Diſſertation in Morgagni adverſar.
(*b*) H[illegible]e, Element. Phyſiolog. Human. T. iv. p. 203.

Ganglions befides, inftead of being inftruments fubfervient to the will, are almoft peculiar to nerves, diftributed to parts, the motions of which are totally involuntary: And our author muft indeed have been greatly mifled by his hypothefis not to obferve this ftriking circumftance.

The theory, which prevailed in his time and country, of the action of the dura mater upon the brain, now exploded, might lead this great man more entirely to believe an analogous mufcular power in Ganglions. But the brain needs no mufcular force to imprefs motion upon the animal fpirits; and granting Ganglions to be, as is ingeniously conjectured by Lancifi and Winflow, fubfidiary brains, or analogous to the brain in their office, neither will they need any fuch mufcular apparatus and force. A power, in fine, abfurd no lefs than chimerical, as it fuppofes the force of mufcles of the greateft exertion and effect, to be derived from thofe of leaft bulk and ftrength (which muft be in fome proportion to the quantity of mufcular fibres); and would be a fingle inftance of a mechanical force producing another infinitely greater than itfelf.

1. Ganglions are obferved to be formed generally upon nervous cords formed by the union of feveral different nerves. 2. They appear to abound with blood veffels. 3. The bulk of a Ganglion exceeds, for the moft part, that of all the nerves and veffels, which it receives, and of which it may feem compofed (*c*). Hence we may not unreafonably conclude, that

(*c*) Gangliorum moles major eft quam fit aggregatum omnium vaforum ingredientium atque egredientium; quo fit, ut ad eorum productionem, neceffe fit concurrere, præter communia vafa, peculiare aliud corpus, non tam ex cohærentia et

that in Ganglions the different nervous filaments are very intimately mixed, a new nervous organization, or modification of the medullary ſubſtance, may take place, ſo as to ſecern new animal ſpirits, or alter the direction of thoſe already brought thither; a conjecture, which has the ſanction of the lateſt as well as the earlier thoughts of the great Morgagni (*d*).

In order to determine the particular uſe of Ganglions (the intimate ſtructure of which, equally with that of the brain and medullary ſubſtance of the nerves, we are hitherto ignorant of) in the animal ſyſtem; let us try, if ſomething tending this way may not be ſuggeſted, by reflecting on the functions and motions of the parts applied principally by nervous cords from below the Ganglions.

The intercoſtal or great ſympathetic nerves abound moſt of all others with Ganglions (*e*); and by examining what is peculiar in the motions of parts, to which theſe nerves are diſtributed, we ſhall probably be led to the uſes of Ganglions.

The muſcular ſubſtance of the heart has its principal, or rather all its nerves, from the intercoſtals;

complicatione præfatorum nervorum, ac ſanguiſerorum, quam ex novis organicis partibus, quas provida ſolerſque natura, ſubſiſtentibus probeq excoctis liquidis, ſimul etiam elongatis, varieque diſpoſitis, ſolidorum fibris, fingat et creat. Lanciſi, de Gangliis, loc. cit.

(*d*) See Morgagni, adverſar. Anatom. ii. p. 71. And De Sedibus et Cauſis morborum, Epiſt. xii. art. 14. p. 95, vol. I.

(*e*) Super omnes nervos, intercoſtali, Ganglia ſunt frequentiſſima, in cervice quidem tria; in thorace, lumbis, et pelvi tot, quot nervorum ex ſpinali medulla propagines intercoſtalis accipit: tum in cordis vicinia, ſub diaphragmate, circa arteriæ cœliacæ et meſentericæ originem: et circa renem paſſim in plexuoſis retibus. Haller, Elem. Phyſ. T. iv. p. 202.

which

which are always detached from the principal cords below the Ganglions, and chiefly from the inferior cervical Ganglion. The few nervous cords from the par vagum or 8 pair, which in the human ſubject are ſent towards the heart, are almoſt totally ſpread upon the pericardium and great veſſels (*f*).

In the abdomen this nerve unites with the par vagum of the right ſide (*g*), and together form the great ſemi-lunar Ganglion; from which, and from other Ganglions formed in inferior parts of the abdomen, filaments are diſtributed to the inteſtines, the liver, the ſpleen, the kidneys; and ſome of them deſcend to the Fallopian tubes, uterus, and other parts in the pelvis; ſome of which are alſo in part furniſhed with filaments from the lumbar nerves.

The heart and inteſtines being wholely ſupplied by nervous filaments detached below ſome remarkable Ganglion, we muſt inquire what is particular in the motions of theſe parts, or in their ſtructure: But the motions of the heart and inteſtines are remarkable, and exactly analogous in being involuntary, or not liable to be either ſtopped, renewed, or in any way controuled by the will.

Tho' it be very certain that theſe motions are excited in the heart by the gentle ſtimulus of the blood upon the infernal ſurface of that organ; and in the inteſtines by that of the ſecreted liquors, and of the food taken in; of which ſtimuli theſe parts have the quickeſt and moſt exquiſite perception: yet this being ordinarily not ſo ſtrong as to make us conſcious of its action, much leſs painfully ſo, can hardly be

(*f*) Haller, Elem. Phyſ. T. i. p. 366.
(*g*) Winſlow, Traité des Nerfs, N°. 141.

ſuppoſed

fuppofed to render thefe motions quite uncontroulable by the will, without fome other efficient caufe (*b*).

Anatomy difcovers no peculiarity in the mufcular ftructure of thefe parts likely to account for this; and, excepting in their nerves having Ganglions, which feem indeed almoft appropriated to them, no anatomical difference has been obferved, no mechanifm, which thefe parts have more than thofe mufcles, which are fubject to the direction of the will.

May we not then reafonably conclude, that Ganglions are the inftruments, by which the motions of the heart and inteftines are, from the earlieft to the laft periods of animal life, rendered uniformly involuntary; and that to anfwer this purpofe is their ufe, which they fubferve by a ftructure unknown to us, no lefs than that of the brain, though it feems not improbable the firft may be analogous to the laft?

This conclufion concerning the ufe of Ganglions is fupported by every truly parallel inftance. Thus the motions of the uvea, or mufcular circle of the eye, ever contracted or dilated as the eye is more or lefs irradiated with light, are as much involuntary as thofe of the heart; and it is known to anatomifts, that the mufcular fibres of the uvea are fupplied by nerves from the lenticular Ganglion, which feems

(*b*) In the beft explanation of the vital and involuntary motions, which the public has been favoured with, it is remarked by the moft ingenious author, "I imagine, that the mind's want "of power over the motion of the head is not only owing to "its being continually acted upon by a ftimulus, but in part to "an original conftitution; and that, tho' we fhould fuppofe this "organ for a little while free from every degree of irritation, yet "the mind, by an effort of the will, could not move it." Dr. Whytt's Effay on the vital and involuntary motions of Animals, p. 316.

formed ſolely for the uſe of that muſcle, and for that purpoſe.

That the determinations of the will are, as it were, intercepted, and prevented from reaching certain parts of the body, by the means of Ganglions, may be farther inferred by conſidering, that all nerves, which have a ready communication with the ſoul, either by affecting it with perceptions, or conveying its commands, have no Ganglions: Theſe are never found upon the olfactory, optic, or auditory nerves, any more than upon the nerves and inſtruments in voluntary motion. For we may well imagine the ſame mechaniſm, which prevents the will from extending its controul to ſome muſcles, placed upon a ſenſory nerve, would have equally hindered the conveyance of any ſenſible impreſſion to the mind.

The left nerve of the eighth pair, diſtributed to the ſtomach, and probably the cauſe of the diſtinct and exquiſite ſenſation of that organ, and perhaps alſo principally concerned in tranſmitting the ſenſe of hunger to the mind, may therefore be reckoned a ſenſory nerve. It is certain alſo, that Haller (*i*) and moſt modern anatomiſts do not allow any Ganglion to this nerve, though Winſlow does, and Vieuſſens delineates one not far from the great Ganglion of the intercoſtal nerve as proper to the eight pair.

If muſcles ſubject to the will might have been totally ſupplied with nerves, which have Ganglions; the diaphragm had probably been entirely furniſhed from the intercoſtals, as moſt of the parts in the thorax above it and in the abdomen below it are. But as the motions of this muſcular membrane were to be

(*i*) A. Haller, Primæ Lin. Phyſiolog. N°. 377.

controulable

controulable by the will, we find peculiar nerves, namely the phrenic, which have no Ganglions ſent to it from a great diſtance.

In propoſing this as the probable uſe of Ganglions, I am far from thinking it entirely exempt from difficulties; but they are chiefly ſuch, as ariſe from our imperfect knowledge of the nerves in general; a terra incognita, which remains to immortalize the name of ſome future diſcoverer in anatomy. It is well known, for inſtance, that all the nerves ſent from the ſpinal marrow have Ganglions, where they ſend off the branch, which communicates with the intercoſtals (*k*). Though this be true, it is moſt probable, that theſe Ganglions reſpect the intercoſtals, and only affect its nerves, leaving the other fibres fit and free for the conveyance of the commands of the will, as in fact many of them are diſtributed to muſcles under its power and direction.

So likewiſe we are not to imagine, wherever the nerves unite, that their medullary ſubſtance either decuſſates, or is ſo intimately mixed, as is reaſonably ſuppoſed to be the caſe in Ganglions by moſt anatomiſts from Lanciſi down to Haller. We know at leaſt, that this is far from being the caſe in the optic nerves; for though they unite, and were ſuppoſed to croſs each other, the contrary appears by obſervations made in the bodies of perſons, who were blind of one eye, from a fault in the optic nerve, the nerve of the affected ſide only being waſted, while the other was large and plump (*l*). And we may juſtly infer the

(*k*) See the table of Vieuſſens.

(*l*) See Monro's Anatomy, p. 356; alſo N° 23, of the nerves in general.

plexi-

plexiform unions of the nerves diſtributed to the ſuperior extremities not more intimate, nor to ſerve any ſuch purpoſe as Ganglions, ſince theſe nerves are equally motory and ſenſory, no other nerves being diſtributed to the ſkin, the organ of touch, but from the ſubdiviſion of theſe plexuſes.

Kidderminſter,
Nov. 24, 1763.

James Johnſtone, *M. D.*

XXXIV. *An Account of ſeveral fiery Meteors ſeen in* North America: *In a Letter to* John Pringle, *M. D. and F. R. S. from* John Winthorp, *Eſq*; Hollſiſian *Profeſſor of Mathematics and Philoſophy at* Cambridge, *in* New England.

S I R,

Read June 7, 1764.

I Am greatly indebted to you for the honour you have done me in ſending me your curious account of the late fiery Meteor in Britain, which I received through the hands of my very worthy friend Dr. Franklin. I have peruſed the account with great ſatisfaction, in which, as it appears to me, you have determined the figure, magnitude, height, path, and velocity of the Meteor, with as much exactneſs as the nature of the thing will admit of. The circumſtances you mention, p. 259, 260, will, I am afraid, always prevent the attainment of the preciſion one could wiſh for in thoſe particulars, ſo neceſſary for laying a ſure foundation to build a theory upon. The hypotheſes hitherto advanced are liable to great difficulties, and the hints you have given in the concluſion towards another appear intirely new, and free from ſeveral objections with which the others are embaraſſed; and I ſhould be very glad to ſee them thrown together into a juſt ſyſtem. If any obſervations ſhould occur to me, that might throw light on this difficult ſubject, I will do my ſelf the honour to communicate them

 to

to you; and ask leave, for my own information, just to query, what center these bodies may most probably be supposed to revolve round? Either the Earth or the Sun seem to bid fairest for this.

1. A body revolving round the Earth in a circle, at the height of about 40 or 50 miles, would move but 7 miles in a second; and in a very eccentric orb, near a parabola, but 10 miles: which falls much short of the velocity of your Meteor, which was of 30 miles in a second *.

2. The Earth's annual velocity round the Sun is near 16 miles in a second; and a body revolving in a very eccentric orb would have, at the same distance from the Sun, a velocity of 22 miles in a second. Wherefore if the Earth and such a body near it were moving in the same direction, that body would get before the Earth with a relative velocity of 6 miles in a second; but, if moving the contrary way, it would be left behind with a relative velocity of 38 miles in a second: And this is the greatest possible relative velocity. Bodies moving in oblique directions may have any relative or apparent velocity less than this maximum. This supposition therefore agrees better with a velocity of 30 miles in a second, than the former; but I shall be much obliged to you for your thoughts on this point.

As to our late Meteor of May 1759, I have not been able to come at any farther particulars than what are contained in my letter to Dr. Birch, excepting only as to the loudness of the report, at a great distance from the place of explosion. An intelligent man of this town has since informed me, that he was then fishing in a boat at anchor about a

* Vid. Philos. Trans. Vol. LI. p. 263.

mile

mile below the light-houſe at the entrance of Boſton harbour, and heard an uncommon noiſe, which was ſomewhat like that of a very hard clap of thunder at a great diſtance, tho' there was not a cloud to be ſeen. All the company in the boat were ſo ſtartled at it, that they left off fiſhing to attend to it; and the noiſe increaſed to ſuch a degree as amazed them all. He ſays, it ſeemed like a continued ſucceſſion of volleys of ſmall arms. He thinks it laſted about 3 minutes, and gradually went off towards the South-Eaſt. They took it for the noiſe of an earthquake, and expected to find every body talking of one, when they got aſhore; tho' they could not perceive the leaſt agitation in their boat.

Had this Meteor happened in the evening, inſtead of the day-time, it would have had many more obſervers; and the brightneſs of it would probably have been thought as extraordinary as the noiſe.

On this occaſion I take the liberty to give an account of two or three other Meteors of this ſort, ſeen in North America; which, if they are permanent revolving bodies, according to your hint, p 273. may poſſibly be of ſome ſervice hereafter in enumerating them. The firſt mentioned in the incloſed paper I ſaw in this town; the ſecond, I received an account of in a letter from the Rev. Mr. Clap, Preſident of Yale College in New-Haven, who heard the noiſe himſelf, though he did not ſee the light; and the account of the third, here tranſcribed in the very words of the original, was given me at St. John's, Newfoundland, by Michael Gill Eſq; Chief Judge in the courts there, when I went thither to view

view the tranſit of Venus laſt June. None of theſe accounts are circumſtantial enough to aſcertain the neceſſary particulars of magnitude, height, and velocity. All I can collect from them is, that the height muſt have been very great, and conſequently the explosion very great likewiſe, to produce ſuch a report in a highly rarified medium.

Allow me, Sir, the honour of ſubſcribing myſelf, with very great reſpect,

Your moſt obedient

Cambridge, New-England, November 17, 1761. Humble ſervant,

John Winthrop.

Account of Three Meteors *ſeen in* North-America.

Read June 7, 1764. I. ON the 3d of June 1739, as I was walking over the common in this town [Cambridge] about 10 o'Clock in the evening, the Moon, which was newly paſt the firſt quarter, ſhining bright, and but few clouds to be ſeen, I was on a ſudden ſurpriſed with four or five flaſhes of light, ſucceeding each other as quick as poſſible. This I at firſt took to be lightening; but, looking up, preſently diſcovered the cauſe of it, which was a large Meteor moving almoſt in the meridian from South to North. The body of it was very bright, and left behind it ſeveral

ſeveral ſparks or leſſer balls of light. When I firſt ſaw it, it was not far from the zenith, from whence it moved, not very ſwiftly, till at about the height of 30° above the horizon it expired. In about 2′, it was followed by an hollow rumbling noiſe, pretty loud, and ſo much like remote thunder, that ſeveral perſons in their houſes, who did not ſee the Meteor, took it to be thunder; as others, within doors, who ſaw only the flaſh, and not the body of the Meteor, thought it lightened. But as there was no thunder nor lightening before or after, nor any clouds likely to produce them, as I was well aſſured, being then abroad, I queſtion not but this report was occaſioned by the exploſion of the Meteor. And this is confirmed by the great extent of this ſound, which was heard in ſeveral places above 80 miles diſtant from each other. And from hence, as well as from the length of time between the light and the noiſe, it may be collected that the Meteor muſt have been very high in the atmoſphere.

II. A Meteor on the 24th of November 1742, in the Southern parts of New England.

In New Haven, in Connecticut, one man ſaw a ball of fire about 4 or 6 inches in diameter, paſſing along from the South-weſt to the North-eaſt, and a ſtream of white, bright, and clear fire followed it, of near the ſame bigneſs, and of conſiderable length. Then the ball broke into ſundry ſmall pieces, and vaniſhed with a kind of flaſh; and, a full minute after, he heard a noiſe, much like that of rumbling thunder, and, he ſays, about as long again as a clap of thunder uſually is.—Sundry people at Rehoboth, in this province [Maſſachuſetts] ſaw a ball of about

 a foot

a foot diameter, toward the West from them, and it fell to the ground. — At New-London, in Connecticut, the stream of fire appeared in the North or North-west; and some who were off at sea, near New-London, took the noise to be from great guns at New-London battery. — Mr. Clap observes, that, though the informations he had received differ as to particular circumstances, thus much in general seems to be certain, that people in most, if not all the towns between Norwalk, near the West end of Connecticut, and Braintree near Boston, which is at least 200 miles, heard an unusual noise in the air, like thunder or the discharge of a cannon; and sundry people, in most places, about a minute or more before the noise, saw a ball or stream of fire in the air, moving in some form or other. It was cloudy in the morning, but about 11 of the clock, when this phænomenon happened, it was generally clear, and but few clouds to be seen.

III. A Meteor on the 4th of May 1760, at Newfoundland.

The deposition of James Cawley, master of the Sloop Content, taken before Michael Gill Esq; one of his Majesty's Justices of the peace for the district of St. John's, Newfoundland, sayeth, that, coming from the Banks of Newfoundland for this harbour of St. John's, being Sunday the fourth instant, about a quarter before twelve o'clock at night, being calm and the weather very clear and fair, then, near the mouth of this harbour, a sudden light shined, at which time we saw a fiery Comet or Meteor in the air, at first appearing in the shape of a flask or Florence bottle, which as it came nearer to us still increased in

in magnitude, making the air very hot, ſhooting from the North-ward to the South-weſt; ſparks of fire darting from it, the bigneſs of a man's fiſt. It came very near us before it diſappeared, when it ſeemed to us to be of the bigneſs of a ſhip's boat, with a long tail extending from it, attended with a noiſe like thunder. As it came near the water, the body appeared as black as pitch and then vaniſhed; the tail remaining ſome minutes before it diſappeared.

Signed,

James Cawley.

Sworn before Michael Gill,
the 15th of May, 1760.

The above was likewiſe atteſted upon oath by the ſubſcribers, being ſailors on board the ſaid veſſel, and at that time upon deck.

John Sullivan,

[A true Copy.]

Mark
Thomas + Frog.
of

The teſtimony of Richard King, which he is ready to make oath to, ſaith, that coming from Kitty-Vitty to St. John's on Sunday the 4th of May, between the King's Bridge and the Garriſon, he ſaw towards the garriſon as if there was a ſtar ſhooting, falling greatly, but only it made too large *. It was as large as a man's head, and juſt before it came to the ground it broke all to pieces, which made like large ſparks of fire flying from it; and in that time it was as light as ever he ſaw all day: And in leſs than two or three minutes there was a rumbling noiſe in the air, ſomething like thunder.

Several other perſons in St. John's were prodigiouſly ſurpriſed at the ſame light.

* His meaning ſeems to be, it was too large for a ſtar.

XXXV. *Some New Properties in Conic Sections, discovered by* Edward Waring, *M. A.* Lucasian *Professor of the Mathematics in the University of* Cambridge, *and F. R. S. to* Charles Morton, *M. D. Sec. R. S.*

THEOR. I.

Read June 21, 1764.

SIT ellipsis APBQCRDSET, &c. describantur circa eam duo polygona [TAB. XIII. Fig. 1.] (*abcdef*, &c. *pqrstv*, &c.) eundem laterum numerum habentia, & quorum latera ad respectiva contactuum puncta (APBQCRDS, &c.) in duas æquales partes dividuntur, i. e. $aA = Ab$, $bB = Bc$, $cC = Cd$, &c. $pP = Pq$, $qQ = Qr$, $rR = Rs$, &c. & erit summa quadratorum ex singulis unius polygoni lateribus æqualis summæ quadratorum ex singulis alterius polygoni lateribus, i. e.

$ab^2 + bc^2 + cd^2 + de^2 + ef^2 +$, &c. $= pq^2 + qr^2 + rs^2 + st^2 + tv^2 +$, &c.

Cor. Ducantur lineæ AB, BC, CD, DE, EF, &c. PQ, QR, RS, ST, TV, &c. & erit $AB^2 + BC^2 + CD^2 + DE^2 + EF^2 +$ &c. $= PQ^2 + QR^2 + RS^2 + ST^2 + TV^2 +$ &c.

THEOR. II.

Iisdem positis sit O centrum ellipseos, & ducantur lineæ O A, O P, O B, O Q, O C, O R, O D, O S, &c. erit

$OA^2 + OB^2 + OC^2 + OD^2 + \&c. = OP^2 + OQ^2 + OR^2 + OS^2 + \&c.$

Cor. Ducantur etiam lineæ O *a*, O *p*, O *b*, O *q*, O *c*, O *r*, O *d*, O *s*, &c. & erit

$Oa^2 + Ob^2 + Oc^2 + Od^2 + \&c. = Op^2 + Oq^2 + Or^2 + Os^2 + \&c.$

Hæc etiam vera sunt de polygonis inter conjugatas hyperbolas eodem modo descriptis.

THEOR. III.

Sit conica sectio M P Q R S T M' &c. [Fig. 2.] cujus diameter sit A L, et ejus ordinata M L'; sit $Mp = Mv$, & consequenter $Lp = Lv$.

Ducantur lineæ *pq*, *qr*, *rs*, *st*, *tv*, &c. quæ respective tangant conicam sectionem in punctis P, Q, R, S, T, &c. & erit contentum

$pP \times qQ \times rR \times Ss \times \&c. = Pq \times Qr \times Rs \times St \times Tv \times$ &c. vel, quod idem est, summa omnium hujus generis rationum ($Pp : Pq$, $Qq : Qr$, $Rr : Rs$, $Ss : St$, &c.) erit nihilo æqualis.

Cor. 1. Sit ellipsis P Q R S T V &c. circa eam describatur quodcunque polygonum (*p q r s t u w*, &c.), [Fig.

Fig. 1.

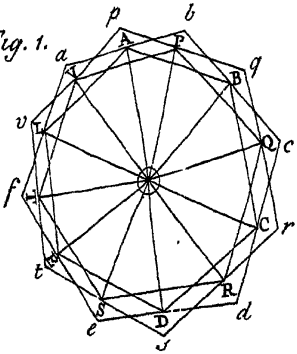


Fig. 2.

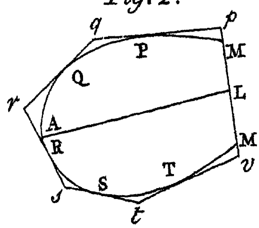


Fig. 3

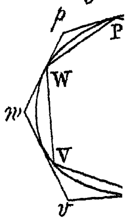


Fig. 5.

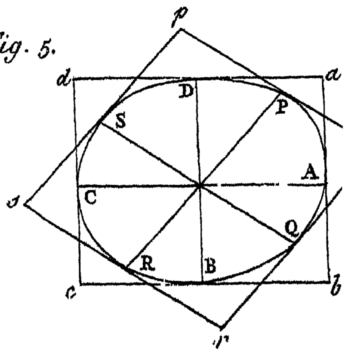


Fig. 6.

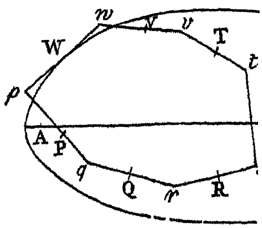


Fig. 4.

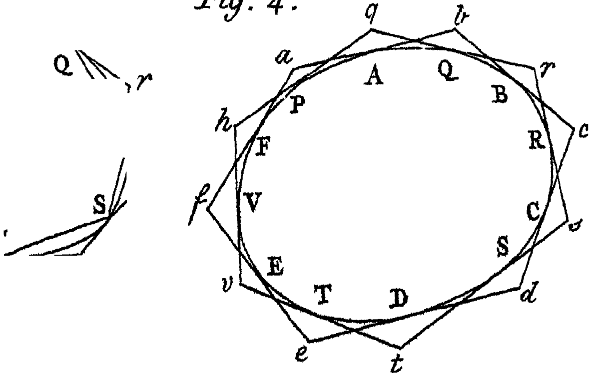
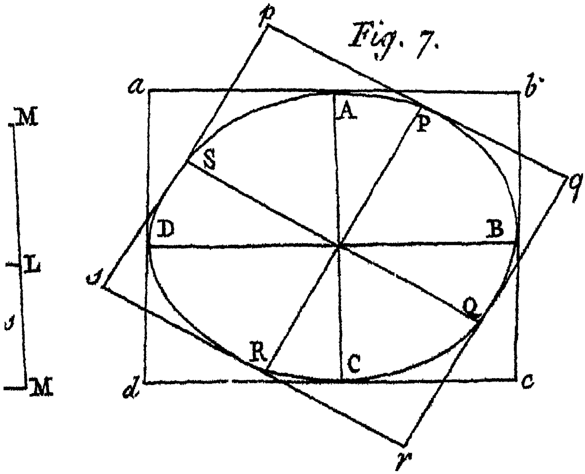


Fig. 7.



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[Fig. 3.] cujus latera reſpective tangant ellipſim in punctis P, Q, R, S, T, V, &c. & erit contentum

$pP \times qQ \times rR \times sS \times tT \times vV$ &c. $= Pq \times Qr \times Rs \times St \times Tv \times Vw \times$ &c.

Cor. Ducantur lineæ PQ, QR, RS, ST, &c. & pro ſinibus angulorum WPp, QPq, RQr, QRr, SRs, TSt, &c. ſcribantur reſpective a, p, b, q, c, r, d, s, &c. & erit

$abcd$ &c. $= pqrs$ &c.

Et ſic de polygonis inter conjugatis hyperbolas inſcriptis.

Idem verum eſt de polygono, cujus laterum ſumma vel area minima ſit, circa quamcunque ovalem in ſeſe ſemper concavam deſcripto, ut conſtat e noſtra Miſcell. Anal.

THEOR. IV.

Sit ellipſis PAQBRCSDTEVF, &c. [Fig. 4.] circa eam deſcribantur duo polygona $abcdef$, &c. $pqrstu$, &c. eundem laterum numerum habentia; eorum latera ab, bc, cd, de, ef, &c. pq, qr, rs, st, tv, &c. reſpective tangant ellipſim in punctis A, B, C, D, E, F, &c. & P, Q, R, S, T, U, &c. & ſit $aA : Ab :: pP : Pq$, & $bB : Bc :: qQ : Qr$ & $cC : Cd :: rR : Rs$ & $dD : De :: sS : St$, & ſic deinceps. Et area polygoni $abcdef$, &c. æqualis erit areæ polygoni $pqrstv$, &c.

Cor. Duo parallelogramma ($abcd$ & $pqrs$) circa datæ ellipſeos conjugatas diametros (AC & BD; PR, QS) [Fig. 5.] deſcripta, erunt inter ſe æqualia.

In hoc casu enim $aA = Ab$, $bB = Bc$, $cC = Cd$, $dD = Da$, & $pP = Pq$, $qQ = Qr$, $rR = Rs$, $sS = Sp$; & consequenter $aA : Ab :: pP : Pq$ & $bB : Bc :: qQ : Qr$, & sic deinceps: ergo per theorema hæc duo parallelogramma erunt inter se æqualia, quæ est notissima ellipseos proprietas.

Idem dici potest de polygonis inter conjugatas hyperbolas eodem modo descriptis.

THEOR. V.

Rotetur conica sectio circa diametrum ejus (AL), & sit MAM', &c. solidum exinde generatum; sint pq, qr, rs, st, tv, vw, wp, &c. [Fig. 6.] lineæ, quæ tangant solidum in respectivis punctis P, Q, R, S, T, V, W, &c. & erit contentum

$$pP \times qQ \times rR \times sS \times tT \times vV \times wW \times \&c. = Pq \times Qr \times Rs \times St \times Tv \times Vw \times \&c.$$

THEOR. VI.

Sit ellipsis APBQCR, &c. rotetur circa diametrum ejus BD; & circa conjugatas diametros (AC & BD, PR & QS) describantur elliptici cylindri (*pqrs* & *acbd*) [Fig. 7.] solidum generatum circumscribentes, & erunt hi duo cylindri inter se æquales.

Sint duo solida e truncatis conis composita, solidum generatum circumscribentibus, & quorum latera continuo

tinuo eâdem ratione ad puncta contactuum dividuntur; erunt hæc duo solida inter se æqualia.

Et sic de solidis inter conjugatas hyperboloides eodem modo descriptis.

Facile constant plures consimiles conicarum sectionum proprietates.

Hujus generis proprietates affirmari possunt de infinitis aliis curvis, ut facile deduci potest e nostrâ Miscell. Anal.

XXXVI. *An Account of the Effects of Lightening at* South Weald, *in* Essex: *By* W. Heberden, *M. D. and F. R. S.*

Read June 28, 1764.

SOUTH Weald is a village in Essex, about eighteen miles distant from London, and two to the North West of Brentwood. In the road from London there is an almost continual ascent for the last four or five miles, which makes a considerable eminence above any parts of the neighbouring country. On the highest part of it stands the church, which has at the West end a tower, and in one corner of this there is a round turret, being a continuation of the stair-case, about four feet wide, eight feet high, and the walls of it one foot thick. In the top of the wall of this turret, which was leaded, are fixed several iron bars, that are bent so as to meet in the middle and support a weather-cock, which was put up about sixteen years ago.

On Monday June 18th, 1764, between twelve and one (about three hours before the time when the thunder and lightening happened in London, by which St. Bride's Steeple and Essex-Street were damaged) there was a storm at South-Weald, attended with uncommonly loud thunder. The lightening struck the weather-cock, and passing along the iron bars, upon which it stands, rushed against the wall of the turret, and has broken a space from the top of the turret to the leads of the tower, about four feet wide, being about one third of the circumference of the turret and facing the North. The weather-cock, and irons that support it seem to be unhurt. The

 walls

walls of the turret were made of rough ſtones and mortar; and part of what is beaten down has fallen upon the leads of the tower underneath, and part upon the roof of the church, which is greatly damaged. The ſtair-caſe alſo, which leads up to the turret, is ſo full of the ſtones and mortar, that it is with great difficulty and ſome hazard that any one can go up it. From a leaden ſpout at this Weſt end of the church, which only comes down to near the top of the Weſt window, the plaſter is beaten off the wall for ſome inches in breadth quite to the window; and at the bottom of the upright iron bars of this window ſeveral of the ſtones are cracked, and the wall is chipped here and there from thence to the ground. The ſame is obſervable in the ſtones at the bottom of the upright iron bars in the Eaſt window, which is alſo near a leaden ſpout that comes down from the roof over the chancel, the end of which reſts upon a buttreſs, and does not reach the ground by ſeveral feet; which buttreſs is cracked, as well as the adjoining wall. On the inſide of this wall, within the church, there is a large wooden frame, which holds the commandments. This frame at the left hand corner is ſupported by an iron holdfaſt driven into the wall, which was mentioned above as being cracked on the outſide under the leaden ſpout. The plaſter of the wall, for three or four inches all round this holdfaſt, within the church, is beaten off; and to the left hand there is a ſpace, ſlanting from the holdfaſt toward the ground, five or ſix inches wide and three or four feet long, from which all the mortar is forced away. That part of the wooden frame, where the holdfaſt is fixed, is ſhattered. The canvas, upon which the

com-

commandments are painted, which was in this wooden frame, is torn from the frame on the two ſides of it next the holdfaſt, and is rent beſides in ſeveral places.

The whole appearance of the damage done to this church very much favours the conjecture of that ſagacious obſerver of nature, Dr. Franklin, who thinks it probable, that, by means of metallic rods or wires reaching from the roofs to the ground, any buildings may be ſecured from the terrible effects of lightening.

XL. *Observations upon the Effects of Lightning, with an Account of the Apparatus proposed to prevent its Mischiefs to Buildings, more particularly to Powder Magazines; being Answers to certain Questions proposed by* M. Calandrini, *of* Geneva, *to* William Watson, *M. D. F. R. S.*

To the Right Honourable the Earl of MORTON, *President of the Royal Society.*

My Lord,

Read June 28, 1764.

I Very lately received a letter from the learned and ingenious Monsieur Calandrini, of Geneva, who has a considerable employment in the Ordnance in that city. In this letter Monsieur Calandrini tells me, that he had perused with attention a letter which I wrote to the late Lord Anson, which contained some suggestions tending, as I hoped, to prevent the mischiefs occasioned by lightning to ships at sea; and which likewise might, on the same account, be useful to powder magazines. This letter was printed in the Philosophical Transactions *. He says, that he has considered with satisfaction the real advantages, which may arise from thence to fortified towns, where the quantity of gun-powder may, from any accident, endanger the whole fabric of a city.

This gentleman therefore is desirous of presenting a memorial to the Board of ordnance at Geneva; in which he would be very glad to explain to their satisfaction the method I propose. He has therefore sent

* Phil. Transf. Vol. LII. page 629.

ſent me the following queſtions, which he thinks tend to throw further light upon this ſubject, and has deſired my opinion upon them. As theſe may poſſibly hereafter be of public utility, I have taken the liberty of communicating them to your Lordſhip.

I. What ſort of apparatus is uſed at Philadelphia?

II. Whether there is not ſome improvement to be made to their methods?

III. In what manner this apparatus may be adapted to powder magazines?

IV. Into what place the thunder may be conducted, where there is no river near, to anſwer the purpoſe of the ſea about ſhips?

V. Whether the apparatus might not electrify the air, ſo as to occaſion lightning, which was, he believes, the cauſe of the death of Profeſſor Richmann of Peterſburg? This apparatus may not be dangerous to dwelling houſes, where the fire may ſlip without any manner of riſk; but may be attended with the moſt dreadful conſequences to a powder magazine, where the ſmalleſt ſpark may occaſion the exploſion of the whole.

VI. Whether the ſquare, or the circular form of building, will be eaſieſt adapted to the apparatus?

VII. Whether an iron bar fixed on the top of the building, to ſupport a weather-cock, may not attract the thunder bolt, and be conſequently dangerous to all buildings; but more eſpecially to powder ma- [illegible]

VIII. Whether there is not ſome particular manner of buildings, invented of late, adapted to powder magazines; either to diminiſh the ſhock of the exploſion, or to ſecure them againſt any accident, by the method uſed at Philadelphia?

Mr.

M. Calandrini ſays further, that he himſelf has been eye-witneſs of the effects of lightning coming into a room, which had received much damage from it. That he looked for the place it went out at, and after long ſearch perceived that it had followed the wire of the bell, which had conducted it through a very inconſiderable hole into the next room; from whence it had opened itſelf a paſſage into a back yard. This accident was at that time thought very extraordinary, being anterior to Dr. Franklin's experiment.

To M. Calandrini's queſtions I have ſent the following anſwers.

I. The apparatus, uſed at Philadelphia, conſiſts either of a long iron rod, placed upon the higheſt part of an houſe, or other building; or, of a ſhorter rod, inſerted into a long wooden pole, placed in the ſame manner. The iron rod, mentioned by Mr. Kinnerſley in the Philoſophical * Tranſactions, and which probably preſerved the houſe in Philadelphia upon which it was placed, extended in height about nine feet and a half above a ſtack of chimnies, to which it was fixed; but he ſuppoſes that three or four would have been ſufficient. Theſe rods are pointed at their upper extremity. It is indifferent, which of theſe two are uſed, provided that they are of height enough to reach above the chimnies, or any other part of the edifice. Connected to, or ſuſpended from, the metal of theſe, a metallic wire, generally of iron, is conducted, in the eaſieſt and moſt convenient manner, to the neareſt water, viz. to the well of the houſe, or any other water in the neighbourhood.

* Vol. LIII. page 95.

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II. This method, wherever it has been employed, has hitherto perfectly anſwered the intention; no houſe in Philadelphia, or in any other place I have heard of, having ſuffered from the effects of lightning, where this apparatus has been erected. The improvements I ſhould recommend would be, firſt; that, as iron wire ſoon becomes ruſty, and when ruſty to the center is unfit for the preſent purpoſe; and as braſs wire is, when long expoſed to the weather, exceedingly brittle and liable to ſnap aſunder, the wire ſhould be of copper; and of a ſize not leſs than that of a large gooſe quill. Secondly, I prefer it's being conducted, from the rod at the top to the water below, on the outſide of the building, and thereby prevent the lightning from coming within the building. On houſes, where there are gutters and ſpouts of lead to carry off the rain, the wire need only be conducted to the lead of the gutters; and attention be had that the gutters and the ſpouts coming from them are in their whole length in contact, or very nearly ſo, one with the other. If the leaden ſpouts do not reach to the bottom of the building, a ſlip of lead, ſuch as is employed for the gutters, and about an inch wide, ſhould be faſtened to the bottom of one or two of the ſpouts, and conducted to the water. If a ſlip of lead, ſuch a one as has juſt been mentioned, was to be conducted from the rod at top to the gutters, it might with equal advantage be ſubſtituted for the copper wire: or further, a ſlip of lead of this kind may be connected with the rod at the top of the houſe; and, where there are no leaden gutters or ſpouts, may be conducted on the outſide of the houſe down to the water, as I before mentioned.

oned. I would recommend likewiſe an increaſe of their number; as the effects of one apparatus of this kind can extend only to a certain diſtance, and that to no great one; and the ſecurity, where miſchiefs from lightning are frequent, muſt ariſe from their number. In countries and places ſo circumſtanced, no houſe or other building ſhould be without one at leaſt; large edifices ought to have ſeveral. The number ſhould be in proportion to the ſize of the building.

III. In powder magazines, I ſhould recommend the apparatus to be detached from the building itſelf; and to be only placed as near it as might be. Powder magazines ſhould never be conſtructed ſo, as to cover a large quantity of ground. If ſecurity from lightning was conſidered in their conſtruction as a conſiderable object, I ſhould recommend a circular building; in the periphery of which ſhould be placed ſtorehouſes ſufficient in their number and extent to contain the quantity of powder propoſed. In the centre of this circle ſhould be a well, very near which ſhould be erected a pole or maſt, high enough to reach ſome feet above the buildings of the powder magazine, or the buildings in it's neighbourhood. From this maſt there ſhould riſe a braſs rod, five or ſix feet in length, an inch in thickneſs, and ending in a point; and from this rod a wire of copper of a ſize not leſs than that of a large gooſe quill, ſhould be conveyed down the maſt, and terminate in the water of the well. If there is no well, the wire ſhould be laid into the neareſt water; as the expence even of ſome hundred yards of a wire of this ſort can hardly be conſidered as an object in an affair of this importance.

tance. For though I have reaſon to believe, that the wire communicating with the ground would prevent the miſchiefs of a thunder cloud, which came near an apparatus of this ſort; yet as water is a more ready conductor than the ground, it ſhould, if poſſible, be inſiſted upon in this particular caſe, and employed. Mr. Weſt's apparatus, deſcribed by the before-mentioned Mr. Kinnerſley, terminated in an iron ſtake, driven four or five feet into the ground; nevertheleſs the earth did not conduct the lightning ſo faſt but that, in a thunder ſtorm, the lightning was ſeen to be diffuſed near the ſtake two or three yards over the pavement, though at that time very wet with rain. It is preſumed, that had this iron ſtake been placed in water inſtead of earth, the lightning had not been viſible, on account of the water's receiving the electric matter more readily than earth. Where this apparatus therefore is applied to powder magazines, it ſhould certainly terminate in water. At Mr. Hamilton's at Cobham, about twenty miles from hence, where an apparatus of this ſort was erected upon an high and greatly-expoſed building, as there was no water but at a great diſtance, the bottom of the wire was placed deep in an hill of moiſt ſand. If, inſtead of one wire, two, three, or more, were adapted to the braſs rod in this manner, and conducted to the water, or if the braſs rod itſelf was continued to the water, I ſhould conſider it, in extraordinary caſes, as an additional ſecurity. This will explain my ſentiments upon the third, fourth, and ſixth queſtions.

V. As the expectation of the utility of this apparatus is preſumed to be the preventing of the accumulation of electricity in its neighbourhood, by affording

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ing a conſtant and eaſy paſſage to the electricity of the clouds ſurcharged therewith, nothing, in my opinion, need be apprehended from the apparatus electrifying the air; as its principal operation is conceived to be the reverſe of that, viz. diveſting the air of it's electricity. I am well apprized from experiments made here, that the earth is frequently electrified *plus*, and the clouds *minus*; and that this change of *plus* and *minus* between the clouds and earth are ſometimes ſeen to vary ſeveral times in a quarter of an hour: but in that caſe it is preſumed, that the clouds, within the ſphere of action of the apparatus, have by it's operation their electricity brought to the ſame ſtandard with that of the earth in its neighbourhood, and *vice verſâ*; and conſequently, that the miſchiefs which might ariſe from the difference of the denſities of the electricity in the earth and clouds are prevented, by the equilibrium between them being maintained. This ſubject, in relation to the electricity's being *plus* or *minus*, I many years ago conſidered, and laid my thoughts thereupon before the public, as may be ſeen in the Philoſophical Tranſactions, Vol. XLV.

That the atmoſphere at times is very ſtrongly electrified, is evident, to ſay nothing of lightning, not only from our apparatus, but from the maſts of ſhips, being beſet with *St. Elmo's fires*, which I believe would ſcarce, if ever, happen, were the maſts provided with an apparatus of this ſort; unleſs the cauſe might be ſo great, and come on ſo faſt, that the metal employed between the tops of the maſts and the water might not, on account of the vaſtneſs of the cauſe, be large enough for the purpoſe. If it ſhould ſo happen, *St. Elmo's fires* might ſtill appear

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at the tops of the masts, and thunder clouds might burst near them, and exert their dreadful effects *. That even artificial electricity, when in too great a quantity, and hurried on too fast through a fine iron wire has a remarkable effect upon the wire, appears from a very curious experiment of Mr. Kinnersley of Pensylvania. This gentleman in the presence of Dr. Franklin, by *his case of bottles* being electrified fully, and made to explode at once, after the manner of the experiment of Leyden, through a fine iron wire, the wire appeared at first red hot, and then fell into drops, which burned themselves into the surface of his table or floor. These drops cool in a spherical figure, like very small shot, of which Dr. Franklin transmitted some hither to Mr. Canton †, who has repeated this experiment. This proves the fusion to have been very compleat, as nothing less than the most perfect fluidity could give this figure to melted iron. These effects from artificial lightning, are exactly similar to those of the natural; as we have several times known iron wires,

* See more upon this subject Phil. Trans. Vol. XLVIII. page 215.

† The diameter of a piece of Mr. Kinnersley's wire, which I received from Doctor Franklin, was one part in 182 of an inch. Artificial lightning from a case of 35 bottles, I find will entirely destroy brass wire of one part in 330 of an inch. At the time of the stroke, a great number of sparks, like those from a flint and steel, fly upwards, and laterally from the place where the wire was laid, and lose their light in the daytime at the distance of about two or three inches. After the explosion, a mark appears on the table the whole length of the wire; and some very small round particles of brass may be discovered, by a magnifier, near the mark; but no part of the wire itself can be found. J. Canton.

nails,

nails, and other metallic ſubſtances to have been melted, and parts of them, while hot, bedding themſelves in wood, by a thunder ſtorm. Of this we had ſome inſtances here in a thunder ſtorm, which happened in July 1759, of which the effects were communicated to the public in the * Philoſophical Tranſactions. As metal has been made red hot, and melted by artificial lightning, how much greater muſt be preſumed to be the effects of the natural; and how much larger ought to be the metallic part of the apparatus, to avert its miſchief? This requires particular attention.

VII. I was of opinion, that iron bars to ſupport weather cocks, if they were placed upon the tops of buildings made of brick or ſtone, and in contact with either of theſe materials, were not dangerous to ordinary buildings on the account you mention, except in very particular and extraordinary caſes; as theſe ſubſtances, when not much heated, conduct the electric matter in a very conſiderable degree. But what lately happened to St. Bride's Steeple, as well as the miſchief to South-Weald church on the ſame day, evinces, to me at leaſt, that the apparatus, uſually applied to weather cocks, ſhould never be truſted in any building, without a metallic communication from them to ſome water, or at leaſt very moiſt ground. St. Bride's Steeple, one of the moſt beautiful in London, was, on Monday, June 18, about ten minutes before three in the afternoon, very greatly injured, in one of the moſt ſevere thunder ſtorms, which ever happened here.

From as attentive an examination, as the ſteeple at the preſent will admit of without ſcaffolding, it

* See Volume LI.

appears to me, that the weather-cock and its apparatus had the principal ſhare in occaſioning the great miſchief done to the upper part of the ſteeple. I am of opinion, that the lightning firſt took the weather-cock and was conducted, without injuring the metal or any thing elſe, as low as where the large iron bar or ſpindle, which is inſerted into the top of the ſteeple, and comes down ſeveral feet of its length, terminates. There the metallic communication ceaſing, part of the lightning exploded, cracked and ſhattered the obeliſk, which terminates the ſpire of the ſteeple, in its whole diameter, and threw off at this place ſeveral large peices of Portland ſtone, of which this ſteeple is built. Here it likewiſe removed a ſtone from its place, but not far enough to be thrown down. From hence the lightning ſeems to have ruſhed upon two horizontal iron bars, which are placed within the building, croſs each other,to give additional ſtrength to the obeliſk, almoſt at the baſe thereof, and not much above the upper ſtory: here, on the North Eaſt and Eaſt ſide,it exploded again at the end of the iron bar, and threw off a conſiderable quantity of ſtone. And here, for the ſake of explanation, I muſt obſerve, that the ſpire of this ſteeple, where it riſes above the bell tower, is compoſed of four ſtories, beſides the obeliſk placed over them. The loweſt and ſecond are of the Tuſcan order; the third is Ionic; and the fourth or uppermoſt Compoſite or Roman. The ſtone piers of theſe ſtories are connected together and ſtrengthened by iron bars placed horizontally near the height of the capitals of the pilaſters, and each ſtory has only one ſet of theſe bars. From the croſs bars near the baſe

of

of the obelisk just now mentioned, the lightning broke through the roof above the Composite story; at the ends of another set of iron bars placed lower than the former, from which it tore out a large portion of the stone. It then struck the iron bars of this story, which are placed immediately under, and in contact with the stones, broke one of the iron bars directly across, and bent the larger part of it from its horizontal direction to near an angle of 45°. Its rapid progress being here in some measure prevented, at the end of one of the iron bars, it threw off the upper part of one of the Composite pillars just above its capital and a large portion of the cornice projecting over it, and that with such a force, that part of a stone which was placed here and formed a portion of the cornice, and weighed seventy two pounds, was projected, not only the whole length of the body of the church, but beyond it, across St. Bride's Lane; where it fell upon the top of an house, and broke through the roof and lodged in the garret. The horizontal distance from the steeple to the place where it fell, was at least 150 feet; the heigth, from which it fell, somewhat more than two hundred. This piece of stone was of a very irregular figure, and must have required an amazing force to rend it, detach it from the building, and throw it to such a distance. The shaft of the pillar, the next to the East of that whose upper part had suffered so much, was likewise violently struck; and a large portion of its diameter broke out and thrown down. The Ionic story has suffered considerably, more particularly the pilaster fronting the North East, and placed directly under the Composite column, whose top was

 thrown

thrown off. This pilaſter is much injured, but the ſtory in general has ſuffered leſs than the Compoſite, and that chiefly where the irons are inſerted; the upper Tuſcan leſs than that, and the lower Tuſcan but little, except in the North Eaſt pier, which is conſiderably cracked and ſhaken; as if in its paſſage part of the force of the lightning was ſpent in theſe exploſions, and part abſorbed and conducted by the maſſes of ſtone. The damage done to the ſteeple is, except near the top, confined almoſt to the Eaſt and North Eaſt ſide, and moſt generally where the ends of the iron bars have been inſerted into the ſtone or placed under it; and in ſome places, by its violence in the ſtone, its paſſage may be traced from one iron bar to another. And it is very remarkable, that, to leſſen the quantity of ſtone in this beautiful ſteeple, in ſeveral parts, cramps of iron have been employed; and upon theſe, ſtones of no great thickneſs have been placed, both by way of ornament and to cover the cramped joint. In ſeveral places, theſe ſquare ſtones have, on account of their covering the iron, been quite blown off, and thrown away. A great number of ſtones, ſome of them large ones, were thrown from the ſteeple, three of which fell upon the roof of the church, and did great damage to it; and one of theſe broke through the large timbers, which form it, and lodged in the gallery.

In the tower of the ſteeple, in the room where the bells are placed, the lightning took the South-weſt window above the bells and cloſe to the window, not far from an iron bar, which goes round, and rent out ſeveral large ſtones; ſome of which fell into the bell, which was very near this part of the ſteeple, and

and was the largeſt in the ſteeple; and, paſſing below the bell, tore out at another place, in a line with the former, a great number more. One of the ſtones, torn out above the bell, was thrown to the North-eaſt ſide of the tower. Between the two places, in which the lightning had here exerted its fury, the wooden block, which confined the axis of the frame of the great bell, and was faſtened down with two iron ſtaples, was thrown off, and the ſtaples torn out. No damage at preſent ſeems done to the bell.

It is remarkable, that, leſs than twenty years ago, one of the ſtones of the obeliſk of this ſteeple was obſerved to be moved from its place, and project ſome inches over thoſe under it. This ſtone was about ſeven feet from the top of the obeliſk. Danger being apprehended from this ſtate of the ſpire, it was taken down to the place where the ſtone was removed, and rebuilt with new ſtone. This accident, at that time, was ſuppoſed to be owing to the ringing of the bells; but it is highly probable, from what has lately happened, that, as that ſtone was removed from its place, very near to that part of the ſpire, where it is now cracked and ſhivered quite a croſs and ſeveral pieces of ſtone thrown down, it was owing to the ſame cauſe as the preſent damage, viz. lightning, though not at that time adverted to.

The lightning on June 18 came from the Weſt and South-weſt; the damage done both to St. Bride's church and South Weald was on the Eaſt and North Eaſt ſides, except that in the bell-loft at St. Bride's. The ſtones both from the ſteeple of St. Bride's and in its tower were thrown to the Eaſt and North Eaſt.

Since

Since the communication of this paper to the Royal Society, the ſteeple of St. Bride's has been ſurveyed, and found ſo very much damaged in ſeveral of its parts, that eighty five feet have been taken down, in order to reſtore it ſubſtantially. Within theſe eighty five feet are comprehended the obeliſk, placed at the top of the ſteeple, the ſmall dome immediatly under it, the ſpace between that and the uppermoſt or Compoſite ſtory, the Compoſite ſtory, and the Ionic ſtory. This laſt, on the Eaſt and North ſides, was taken down to its bottom; but on the other ſides, as they were not injured, ſome parts were permitted to ſtand. Three piers were likewiſe taken down of the ſecond Tuſcan ſtory, and one pier of the firſt. The ſcaffolding to take this down and rebuild it enabled me minutely to examine, not only the damages occaſioned by the lightning, but the manner of its progreſs. This examination confirmed the opinion of the cauſe and manner of this accident, which I communicated to the Royal Society, ſoon after it happened; and before a near inſpection could be obtained. It compleatly indicated the great danger of inſulated maſſes of metal to buildings from lightning; and, on the contrary, evinced the utility and importance of maſſes of metal continued, and properly conducted, in defending them from its direful effects. The iron and lead employed in this ſteeple in order to ſtrengthen and preſerve it, did almoſt occaſion its deſtruction: though after it was ſtruck by the lightening, had it not been for theſe materials keeping the remaining parts together, a great part of the ſteeple muſt have fallen.

The operation and progreſs of the lightning in the obeliſk and upper parts of the ſteeple deſerve more particular attention. To form a more perfect idea of theſe, the following meaſures will in ſome degree contribute.

	Feet	Inches.	
The height of the octogonal obeliſk - - - - - - - - - - - -	22	3	
Length of the iron ſpindle - -	19	9	
Thickneſs of the ſpindle, where inſerted into the ſtone. - - -	0	2	ſquare
Its length inſerted into the ſtone	9	10	
From the bottom of the ſpindle to the firſt cramped joints. - -	5	10	
Three courſes of ſtones without cramps. - - - - - - - - - - - -	5	7	
From the bottom of the ſpindle to the firſt concealed chain. - -	11	5	
From the firſt concealed chain one foot above the baſe of the obeliſk to the firſt croſs chain. - -	2	0	
From the firſt croſs chain to the ſecond, placed in the dome -	8	10	

The vane, the croſs above it, the ball and its ſocket, which covered ſo much of the ſpindle as aroſe above the ſtone, to near ten feet of its length, were of copper gilt. This length of the ſpindle was cylindrical, but the other part was made ſquare, where it began to be inſerted into the ſtone. To faſten this ſpindle more ſecurely in the courſes of ſtone, melted lead had been poured. This lead, in the two lower courſes of ſtone through which the ſpindle had paſſed, not only filled all the ſpace left between the ſpindle and the

ſtones,

ſtones; but had, as it were, ramified itſelf not only between the joints of the ſtones, but had inſinuated itſelf in its melted ſtate into all their ſmall clefts and interſtices. The ſpindle terminated in one ſtone, which occupied the whole area of the obeliſk, and was three feet and near two inches in diameter, and one foot in thickneſs. Into this ſtone the ſpindle was inſerted five inches of its depth, and faſtened by melted lead. Under this ſtone the obeliſk was hollow; but above it was ſolid, excepting the ſpace left for the ſpindle.

Upon examining theſe ſeveral particulars, no injury had been done by the lightening to the vane, its croſs, copper ball, or ſpindle. Of the ſeven courſes of ſtone at the upper part of the obeliſk, and which were above the whole ſtone into which the ſpindle was inſerted, the five upper courſes, though connected together at top and bottom with iron collars ſodered with lead, were not damaged; but the two ſtones, which formed the ſixth courſe, were cracked, ſhivered, and fragments thrown from them. The ſeventh courſe conſiſted likewiſe of two ſolid ſtones. Theſe were burſt from the ſpindle, which was, by the intervention of the lead, connected with them, broke into many parts; each was moved from its place; ſome pieces were thrown down, and one large one projected five inches over the ſtone, immediately under it. The whole ſtone, into which the ſpindle was inſerted, and upon which it reſted, was burſt from the center into a great many pieces, and every piece removed from its place. Some of theſe were thrown from the ſteeple. Several of the larger maſſes of this ſtone, which ſtill cohered, were very much ſhivered. The center of the ſtone,

upon

and near which the ſpindle reſted, was beaten to powder, and a hole made through the under part of the ſtone. That this ſtone in this condition ſhould ſtill ſupport the ſeven courſes above it, which weighed four tuns, excluſive of the ſpindle, vane, and their appertinances; and that the whole did not fall when ſtruck with the lightning, is in no ſmall degree ſurprizing.

From the bottom of the ſpindle to the firſt courſe of ſtone, where the workmen had uſed iron cramps, the diſtance was five feet ſeven inches. Theſe cramps were bedded in the ſtone. Part of the lightning, from the bottom of the ſpindle through the hole juſt now mentioned, ſeized theſe cramps, and threw off large ſcales of ſtones at their ends. From theſe there were three courſes of ſtone, in which there were no cramps; theſe ſuffered nothing.

In edifices of this kind, for additional ſtrength, the builders employ bars of iron, connected together in ſuch a manner as their exigencies require; and theſe, though they have no links, are denominated chains. Theſe are ſometimes ſo adapted to the courſes of ſtone as not to be viſible, and are perfectly concealed: at other times, they are in part viſible, and in part concealed.

The firſt metal, that occurred after the cramps before mentioned, was a concealed chain, one foot above the baſe of the obeliſk, and two feet above the firſt croſs chain. Here two ſtones were burſt and ſhattered. In the courſe of ſtone, where the firſt croſs chain was inſerted, and the ſeveral ſtones connected by iron cramps, many of the ſtones were much ſhattered.

 At

At the baſe of the dome, near nine feet below the firſt croſs chain, was a ſecond. This chain was a double croſs connected at its ends with a circle of iron, which was bedded into the whole courſe and faſtened by melted lead. Here the lightning made great ravage, burſt and threw off the ſtones in which the iron circle was bedded, and tore out part of the roof of the dome, threw off two pieces of the cornice and one of the vaſes, which was contiguous to it. Theſe two pieces of cornice weighed twelve hundred pounds. The courſes of ſtone between the two chains, except thoſe I juſt now mentioned, were not injured.

To what is here ſaid, I ſhall only add, that in no part the ſteeple was injured, except where the ſtones were in contact or very near the iron and lead employed in its building; and the quantity of ſtone burſt, ſpoiled, or ſo much damaged as not fit to be uſed again, amounts, as I am informed by Mr. Stanes, a very honeſt and ingenious maſon, who has contracted to repair the damage done by the lightning, to not leſs than five and twenty tuns. An amazing quantity!

The above mentioned Mr. Stanes was employed, a few years ſince, in the repair of the ſteeple of St. Mary le Bow in Cheapſide, which was injured by a very rare and uncommon accident. At its erection, the builders had employed, near the top of the ſpire, for additional ſecurity, ſeveral iron cramps; the ends of which, by being expoſed to the weather, became ruſty, ſwelled, and ſo much enlarged thereby, as to raiſe the ſtones above them, and to deflect the top of the ſpire ſix inches from the perpendicular. Danger being apprehended from this ſituation, the ſpire was taken

taken down ſeveral feet of its length, and properly repaired. This ought to be a caution to ſucceeding builders, that if, in edifices of this kind, they find it expedient to employ cramps, they ſhould be either of copper, which is not liable to ſwell by moiſture; or, if iron be uſed, ſo much ſpace ſhould be left in the under bed of the ſtones, which immediately cover the cramps, that they may have room to extend themſelves without danger to the building. This remark, though not immediately relating to our preſent purpoſe, will not, I hope, be thought impertinent in this place.

But to return: this thunder ſtorm had been preceded by ſeveral very warm days. The nights had ſcarce furniſhed any dew: the air was quite dry, and in a ſtate perfectly unfit to part with its highly-accumulated electricity without violent efforts. This great dryneſs made the ſtones of St. Bride's ſteeple, and all other buildings under the like circumſtances, far leſs fit than if they had been in a moiſt ſtate, to conduct the lightning, and prevent the miſchief. For though this thunder ſtorm ended in a heavy ſhower of rain, none except a few very large drops fell till after the church was ſtruck; and I have no doubt, but that the ſucceeding rain prevented many accidents of a ſimilar kind, by bringing down with every drop of it part of the electric matter; and thereby reſtoring the equilibrium between the earth and clouds. It is frequently taken notice of, in attending to the apparatus for obſerving the electricity of the clouds, that tho' the ſky is much darkened, and there have been ſeveral claps of thunder at no great diſtance, yet the apparatus will be ſcarce affected by it; but as ſoon

as the rain begins, and falls upon ſo much of the apparatus, as is placed in the open air, the bells of the apparatus in the houſe ring, and the electrical ſnaps ſucceed each other in a very extraordinary manner. This demonſtrates, that every drop of rain brings down part of the electric matter of a thunder cloud, and diſſipates it in the earth and water; and prevents thereby the miſchiefs of its violent and ſudden exploſion. Hence, when the heavens have a menacing appearance, a ſhower of rain is much to be wiſhed for.

From theſe conſiderations, I have no doubt, but that the miſchief done to St. Bride's ſteeple was owing to the efforts of the lightning, after it had poſſeſſed the apparatus of the weathercock, endeavouring to force itſelf a paſſage from thence to the iron work, employed in the ſteeple. As this muſt be done *per ſaltum*, there being no regular metallic communication, it is no wonder, when its force is vehement, that it rends every thing which is not metallic, that obſtructs its eaſy paſſage; and in this particular inſtance, the ravages increaſed as the lightning to a certain diſtance came down the ſteeple. To procure this eaſy paſſage and avert the ravage occaſioned by the want of it, in future, as much as our preſent knowledge in theſe matters will enable us to do, I cannot ſufficiently recommend metallic communications between the metal at the top and water, either as has been before mentioned, or in any other convenient manner, taking care not to be too frugal of the metal employed. This was firſt ſuggeſted by that excellent Philoſopher Dr. Franklin; and ſince much uſed in Philadelphia, and other parts of North America.

Near the ſame time, that the miſchief was done to St. Bride's church, the maſt of his Majeſty's ſhip Ramaillies, lying at Chatham, was ſplit and torn to pieces by the lightening. This is the leſs extraordinary, as, from its height, figure, and conſtituent parts, the maſt of a ſhip ſtops the progreſs of lightening much more than edifices of the ſame height, made of brick or ſtone. This therefore ſeems to require particular attention; but upon this head I fully explained myſelf in my letter to the late Lord Anſon *, and ſhall therefore decline ſaying any thing further of it in this place.

I flatter myſelf, that what has here lately happened will tend to occaſion the applying of an apparatus of this ſort to all buildings, at leaſt, of value and extent. No ſteeple ſhould certainly be without it; and in moſt, if the iron work of the weather-cock can be eaſily got at, it may be adapted with very little trouble or expence. It is only neceſſary to make a metallic communication between this iron work and the lead, which carries off the water. This frequently reaches to the ground or very near it. From the bottom of this, the metallic communication ſhould continue to the neareſt water, or at leaſt to very moiſt ground; though where it can be procured, water ſhould be preferred. Care muſt be taken likewiſe, that metallic communications be added to ſuch parts of the lead, which ſerve to convey the water from the top, as do not already touch or come near each other. And theſe may be either of lead, or of copper wire, ſuch as I have before mentioned. In thunder ſtorms attended with rain, ſufficient in quan-

* Philoſophical Tranſactions, Vol. LII. p. 629.

tity.

tity to run off in ſtreams, a great portion of the electric matter runs off in, and is diſſipated by, theſe ſtreams; and buildings are thereby preſerved from damage.

What happened to St. Bride's cannot but give us ſome apprehenſions for that moſt noble edifice in its neighbourhood; I mean St. Paul's. This is above an hundred foot higher than St. Bride's, and therefore more in the way of accident from thunder ſtorms. Upon its mangnificent lanthorn is placed a croſs of metal, which is inſerted into the ſtone of the lanthorn; and this is ſupported by a truncated cone of brickwork, which ariſes from the arches of ſtone below. The cupola is covered with lead, which is continued to the ſpouts of the ſame material. Theſe bring down the water to the ſtone gallery under the cupola, and end within about a foot of the ſtone. From hence the water is conveyed a conſiderable diſtance, in a ſtone trough or channel, to the leaden ſpouts; and theſe are carried down the building, and terminate, as I was informed upon inquiry, in the common ſewer. By this arrangement the metallic communication is interrupted. In thunder ſtorms during rain, the water carries off in its ſtreams the electricity, as perfectly, as the moſt compleat metallic communication would; but when there is no rain, it is otherwiſe; and theſe interruptions are the great cauſe of danger. To leſſen which, as far as we can conclude at preſent, it would be expedient to make, by the means of ſeveral copper wires, ſmall rods, or pieces of lead, a metallic communication between the gilded croſs, and the lead of the cupola: and again, from the leaden ſpouts of the ſtone gallery

gallery to thoſe, which bring the water thence; care being taken that, from the bottom of theſe laſt, there ſhould be a metallic communication, if there ſhould be found to be none at preſent, with the water in the common ſewer. Thus, without much expence, a compleat metallic communication may be made between the top of St. Paul's church, and the water; which had it been done at St. Bride's, the ravages ſo lately experienced had in all probability been prevented.

From conſidering the circumſtances of this thunder ſtorm, I cannot but be of opinion, that the injury done to St. Bride's prevented miſchief to St. Paul's. St. Bride's is a very high building, and within a ſmall diſtance nearly Weſt of St. Paul's. When this diſtance is conſidered, and that the lightning came in the direction of St. Bride's to St. Paul's, and that when the thunder cloud came near the former, it exploded there, and parted with much of its force; what was left did no damage to the latter, tho' the much higher and more expoſed building, and having a metallic croſs at its top.

I have recommended as metallic conductors copper wires of the ſize of a gooſe quill; as, when of that thickneſs, they may eaſily be bent to any direction; and, where thought neceſſary, any number may be employed. I look upon this as a kind of ſtandard, from what Dr. Franklin wrote to Monſ. Dalibard of Paris upon this ſubject *. He obſerves, in a church which ſuffered greatly by lightning at Newbury in New-England, that though a ſmall wire was beaten to pieces by lightning, and diſſipated by its force, the rod of a pendulum conducted the whole without

* See Phil. Tranſ. Vol. XLIX. p. 305.

being

being melted or otherwiſe injured by it; and that, great as the quantity was in this inſtance, and which utterly deſtroyed the ſmall wire, no damage was done to the building, as far as the ſmall wire, and the pendulum of the clock extended: and in the remarkable inſtance, mentioned by Mr. Kinnerſley in his * letter to Dr. Franklin, where a braſs wire of about two lines thick, ten inches long, and terminating in a very acute point, was inſerted into the iron rod, about two inches and half only of its top were melted by the lightning; the remaining part of it tranſmitting the lightning without being fuſed by it.

You will obſerve in this diſquiſition, that I have no where mentioned the apparatus attracting the lightning. I have avoided introducing the term *attraction* here, operating as an active principle; as I conſider the apparatus purely paſſive, and only affording, from the aptneſs of its parts to that purpoſe, an eaſy and uninterrupted paſſage to the lightning, and thereby preventing its violent efforts.

You will pardon, Sir, this long digreſſion in relation to St. Bride's church; as it gives ſo poſitive and explicit an anſwer to part of your ſeventh queſtion; ſuch a one as could not, without the late thunder ſtorm, have been furniſhed, at leaſt from hence: To wit, that, without a proper apparatus, weather-cocks placed at the tops of any buildings are dangerous to them in thunder ſtorms; but more eſpecially to powder magazines.

The accidents, which have lately happened to St. Bride's and South Weald churches, if conſidered as great electrical experiments, furniſh very important, and, I flatter myſelf, uſeful concluſions. They are too hazardous

* Philoſophical Tranſactions, Vol. LIII. p. 96.

and

and expenſive however, to wiſh to ſee often repeated.

If the erecting of an apparatus of this ſort ſhould become general in countries where thunder ſtorms are frequent and often attended with miſchief, though damage ſhould really be averted by it, the operation of the apparatus would be unſeen, and therefore unknown, unleſs in ſuch rare inſtances as that mentioned by Mr. Kinnerſly. To make its effects apparent, as has been hinted to me by Dr. Heberden, a very deſervedly eminent phyſician here, if chains are employed as metallic communications, inſtead of wires or rods, whenever the lightning comes near enough to affect the apparatus in a conſiderable degree, it will without miſchief be viſible in the dark, by its ſparkling and ſnapping in its paſſage, at the links of the chain.

The effects of the apparatus may be obſerved in another manner. If the metallic communications are by the means of a wire or ſingle rod, there may be, in ſome part of its length, in any place convenient for obſervation, a ſpace left where the metal is diſcontinued; but this ſpace ſhould not exceed two inches. The two extremities of the metal at this interruption ſhould be furniſhed with braſs knobs not leſs than an inch in diameter. By this method, though the effects of the apparatus would not be conſiderably leſſened, they might be obſerved. For at times, when no lightening was viſible, but when clouds replete with it came near the apparatus, or rain from them fell upon it, there would be a ſnapping from one of the braſs knobs to the other. When indeed the lightning was near, there would not only be this ſnapping; but, if the cauſe was great, a ſtream of fire would be

ſeen

ſeen, as in M. Romas's kite*, to paſs from one of theſe to the other, as the beſt and neareſt conductor. If danger however is apprehended, a piece of chain may be always at hand to be hung occaſionally upon the upper knob, ſo as readily to fall in contact with the lower. Otherwiſe, if while the metallic communication is divided, though when entire it is apprehended it may be touched with ſafety, a perſon ſhould touch the rod above the diviſion and at the ſame time touch or come very near the rod below the diviſion with any part of his body; and at the ſame inſtant if a ſmart ſtroke of lightning affected the apparatus, he would certainly be deſtroyed, as happened to profeſſor Richmann at Peterſbourg; the lightning going through his body from one part of the apparatus to the other, which it is believed it will not do, while the metallic communication is compleat.

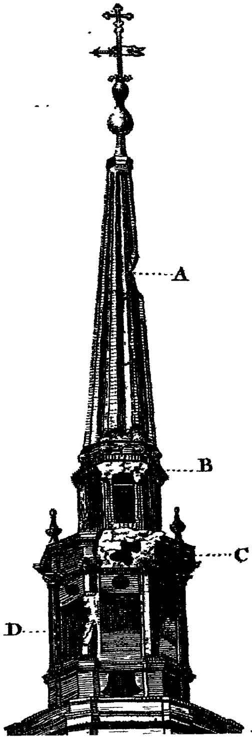
VIII. I have not heard that there has been here of late any particular mode of buildings, adapted to powder magazines, to diminiſh the ſhock of the exploſion in caſe of accident: nor do I believe that any attention has been here given, in conſtructing theſe buildings, to prevent, by an apparatus of this kind, the effects of lightning.

Theſe, my Lord, are my anſwers to M. Calandrini's queſtions. If they are ſatisfactory to that ingenious gentleman, or have the leaſt tendency to public utility, I ſhall be gratified. As I know your Lordſhip's zeal for philoſophical diſcuſſions, I have taken the liberty of ſending you theſe queries and

* Philoſophical Tranſactions, Vol. LII. p. 341.

my


E
F
G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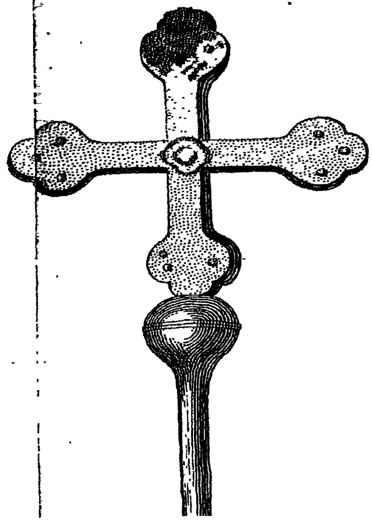

H

my anſwers to them, as a teſtimony of the very great eſteem and regard, with which I am,

My Lord,

Your Lordſhip's

moſt obedient,

humble ſervant,

Lincoln's Inn-Fields,
June 26th, 1764.

W. Watſon.

XLI. *An Account of the Effects of Lightning in* St. Bride's Church, Fleet-ſtreet, *on the* 18th *of* June 1764: *In a Letter to Mr.* Benjamin Wilſon, *F. R. S. from* Edward Delaval *Eſq*; *F. R. S.*

S I R,

Read June 28, 1764.

THE incloſed is an account of the effects of the lightning on the ſteeple and ſpire of St. Bride's church, with drawings [TAB. XIV. XV.] which very accurately expreſs the parts damaged by it.

I thought it would be of uſe, by deſcribing the ſeveral circumſtances of this accident, to ſhew more fully

fully the neceſſity of preventing the danger, that ſuch buildings are expoſed to.

The conſtruction of the ſpire is ſomewhat ſimilar to that of an apparatus purpoſely contrived to draw the lightning from the clouds, as it runs up towards a point, and ends in a metal vane and croſs, the figure of which, as well as the materials that they conſiſt of, ſeem calculated to admit the lightning with the leaſt reſiſtance.

At this place the firſt marks of it are ſeen: at H the top of the copper croſs, which is the higheſt part of the building, the gilding is by the exploſion partly torn off and partly diſcoloured, ſo as to differ remarkably from the reſt of the croſs where the gilding is very well preſerved. Some ſmall pieces of ſolder are melted; and all this part appears as if it had been expoſed to the fire.

The lightning ſeems to have entered here, and to have been conducted as low as the hole at A, by an iron ſpindle twenty feet in length, and two inches in diameter; of which ten feet were ſurrounded by the copper ball, vane and croſs; and the lower half was incloſed in a groove cut through the middle of the ſolid ſtones which compoſed the upper part of the ſpire, and reſted at A on the bottom of that groove, which was ſunk five inches deep into the loweſt of thoſe ſolid ſtones: this laſt mentioned ſtone being three feet broad and one deep. The interval between the ſides of the ſpindle and the groove made to receive it was filled up by melted lead poured in between them.

The lightning accumulated in the metal, having it's paſſage towards the earth ſtrongly reſiſted at this place, has in expanding itſelf formed the hole A, by burſt-

burfting off from the lower part of the fpindle the ftones contiguous to it on that fide.

At each of the angles of the metal, the ftone on which it refted is cracked, which probably was occafioned by the lightning iffuing with greater freedom from thofe parts, than from the flat furface.

No part of the fpindle is in the leaft injured by the lightning, notwithftanding the great quantity which, from it's effects, appears to have been accumulated in it *.

From hence, as low as to the corniche B, it feems to have been conducted along the furface of the fpire, which was wetted by the rain that had fallen in the morning, before the lightning: and having been accumulated in the iron bars B and C, in difcharging itfelf from them, it has made the greateft explofion at this place.

Under this part the freedom of it's paffage feems to have been hindered by all the dry ftonework underneath, which was defended from the rain by the corniches: and it appears from fome experiments which I formerly made †, that dry free ftone, when warm'd to a certain degree (which probably does not exceed the heat which the ftones of buildings acquire in hot weather) refifts the paffage of the electric fluid or lightning fo ftrongly, that with plates of that ftone, inftead of glafs, I performed the Leyden experiment.

* In the year 1750 the ftones furrounding this fpindle were fo much damaged, that there was a neceffity of taking them down and rebuilding that part of the fpire. The caufe of this was not known at that time, it is probable that it was occafioned in the fame manner as the prefent accident.

† Philofophical Tranfactions, anno 1759. p. 83.

Under the corniche, the lightning defcended only by leaping from one iron to another; and at every leap its force feems to have been weakened, and at laft to have been quite diffipated.

On examining the infide of the fteeple beginning from the top, the firft effect of the lightning that appears is a hole in the ftone work at B, beginning immediately above an iron bar which ferved to fupport the top of the window or opening, and running upwards towards the two crofs iron bars: this, when viewed from the outfide of the church, is feen to have fpread round moft of the lower part of the fpire, fo that it feems in great danger of falling.

The next ftroke is about four feet below: at this place four iron bars lie horizontally acrofs the fpire, and are tied together by chain bars which are inclofed in the ftonework: where the end of one of the crofs bars is inferted in the ftone, the lightning has burft open the hole defcribed at C, and, when the fame is viewed at the outfide, a great part of the corniche appears to be broken off.

At D, where the two iron bars ferving to fupport the top of the windows meet and are joined together, the lightning accumulated in them has broken off the pier by which they were inclofed.

At Fig 2, a bar of iron, which ferved to fupport the top of the window in the fame manner as thofe laft mentioned, 21 inches long clear of the ftonework, and half an inch thick, is broke and bent into the pofition expreffed in the drawing; and the ftones immediately above it are fhattered and disjointed.

The fills of two windows of this ftory are torn off from iron bars which lay beneath them.

At

At E, an iron bar N°. 1. about twenty five inches long, was inclofed nine inches deep in the ftonework of the pier, feparating the Eaft arch from the arch next it towards the North: the end of this bar joins at a right angle another bar, N°. 2, which is laid acrofs the arch. The lightning accumulated in the iron (N°. 1.) which was inclofed in the ftone-work, has burft off all the ftone that furrounded it, and part of the pier adjoining. The flaw is continued downwards, as expreffed in the drawing, meeting with fmaller iron cramps in it's way.

At F, the next arch lying immediately under the laft mentioned one, an iron was inclofed in the ftone in the fame manner as the bar at N°. 1. The ftone is torn off from this iron exactly in the fame manner as at N°. 1: but the damage has not reached much further than the ftone which was contiguous to and covered this bar. At the bottom of this arch the fill ftone, which covered fome cramps of iron, is torn off from it's place.

At G, the next arch under this, the force of the lightning feems to have been much diminifhed, a fmall part of one ftone only being broken.

From the wall at the Weft fide of the South window of the belfry fome ftones are thrown down: one chalky ftone in particular is reduced into an impalpable powder, and the wall under the Weft window is almoft covered with the powder: this ftroke feems to have been directed towards the bells one of which is very near the place damaged: the bells have not been examined; nor can they, as I am informed, without danger of fhaking the fpire by their motion.

This

This is the loweſt mark which is left of the effects of the lightning.

In every part that is damaged, the lightning has acted as an elaſtic fluid, endeavouring to expand itſelf where it was accumulated in the metal: and the effects are exactly ſimilar to thoſe which would have been produced by gun-powder pent up in the ſame places, and exploded. Amongſt many other ſtones thrown to a conſiderable diſtance by theſe explosions, one weighing above ſeventy pounds was removed fifty yards Eaſtward from the ſteeple, where it fell through the roof of a houſe.

It is evident that theſe effects would have been prevented, if a ſufficiently large metallic conductor had been extended from the metal at the top of the ſpire down to the earth, communicating with the other metallic parts of the building that lay in it's way.

From ſeveral obſervations which I made on this occaſion, ſuch a communication ſeems neceſſary in buildings of this form. The iron bars, which were fixed in the ſtonework of the Eaſt arches were ſtruck by the lightning, while thoſe in the arches fronting them on the Weſt ſide of the ſame ſtory remained untouched by it. So that I do not apprehend, that a conductor communicating with the Weſt arches only, would have preſerved the oppoſite ones from the damage which they have ſuffered.

When ſuch buildings are expoſed to very large clouds replete with lightning, there is no reaſon to imagine that they will not convey ſome of their contents to other metallic parts of the building at the ſame time as to the metal at the top: for though the

the conductor may be large enough to convey to the ground, from the top, all the lightning that enters that part; yet one such small conductor cannot be supposed to exhaust those immense bodies so quickly, as to disable them from striking at the same time other buildings, or other parts of the same building.

A wire, or very small rod of metal, does not seem to be a canal sufficiently large to conduct so great a quantity of lightning to the earth; especially when any part of it, or of the metal communicating with it, is enclosed in the stone work: in which case, the application of it would tend to increase its bad effects, by conducting it to parts of the building which it might otherwise not have reached.

Dr. Franklyn, from observing that the filleting of gold leaf on the cover of a book conducted the charge of five large jars, reasons that a wire will be sufficient to conduct the lightning from the highest buildings to the earth.

But it appears from an experiment of his own, that a much larger body of metal, when inclosed between small plates of thick looking-glass, is not sufficient to conduct a fifth part of such a charge, without being melted, and bursting to pieces the plates of glass.

And it is remarkable, that in those parts of the church where the effects of the lightning are most conspicuous, the iron was inclosed in a resisting substance similar to the glass surrounding the gold leaf in that experiment.

Wires, instead of conducting the lightning, have frequently been melted by the explosion. So that, I think,

I think, a conductor of metal less than six or eight inches in breadth, and a quarter of an inch in thickness (or an equal quantity of metal in any other form that may be found more convenient) cannot with safety be depended on, where buildings are exposed to the reception of so great a quantity of lightning. These are the only points in which I have ventured to differ from Dr. Franklyn.

I shall not think my time ill employed, if these observations contribute to our security against the effects of lightning. Certainly an inquiry into the properties of the electric fluid, as it furnishes us with the means of preventing such accidents, is far from being an useless speculation. I am,

Sir,

your most humble servant,

Old Palace Yard,
June 28, 1764.

Edward Delaval.

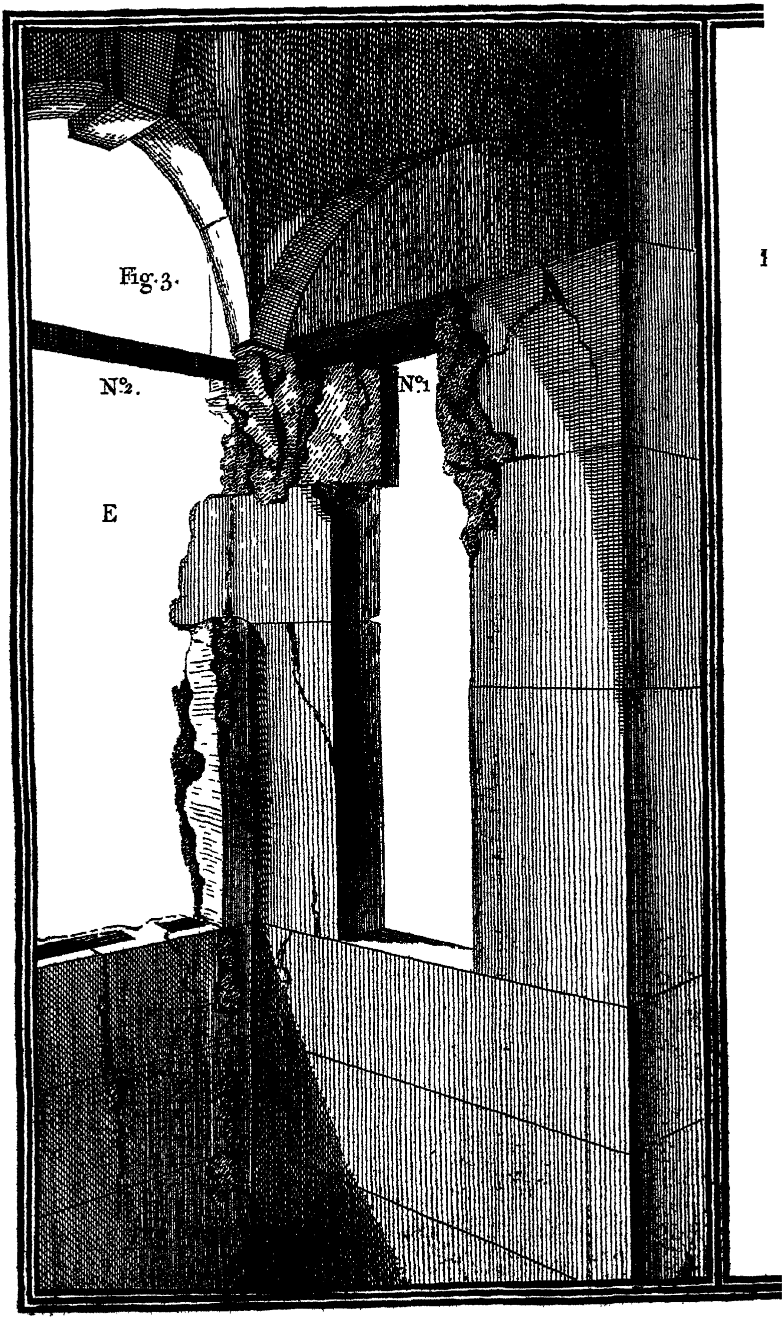
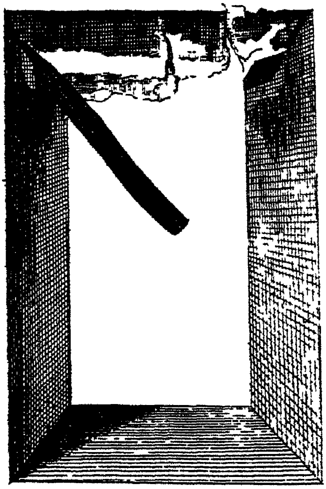
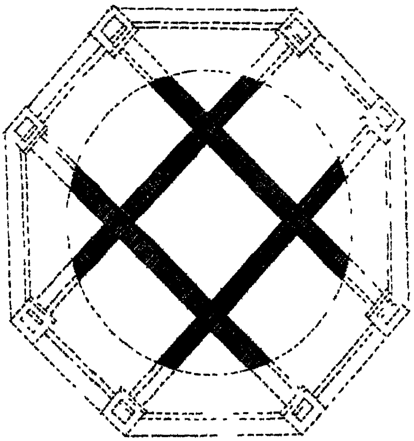


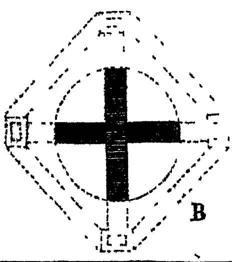
Fig. 3.
N°.2.
N°.1
E

Fig 2.



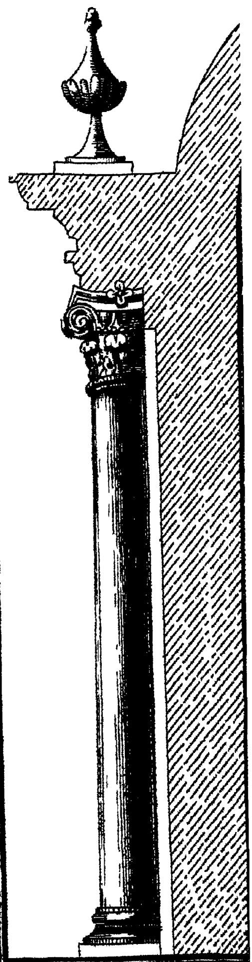


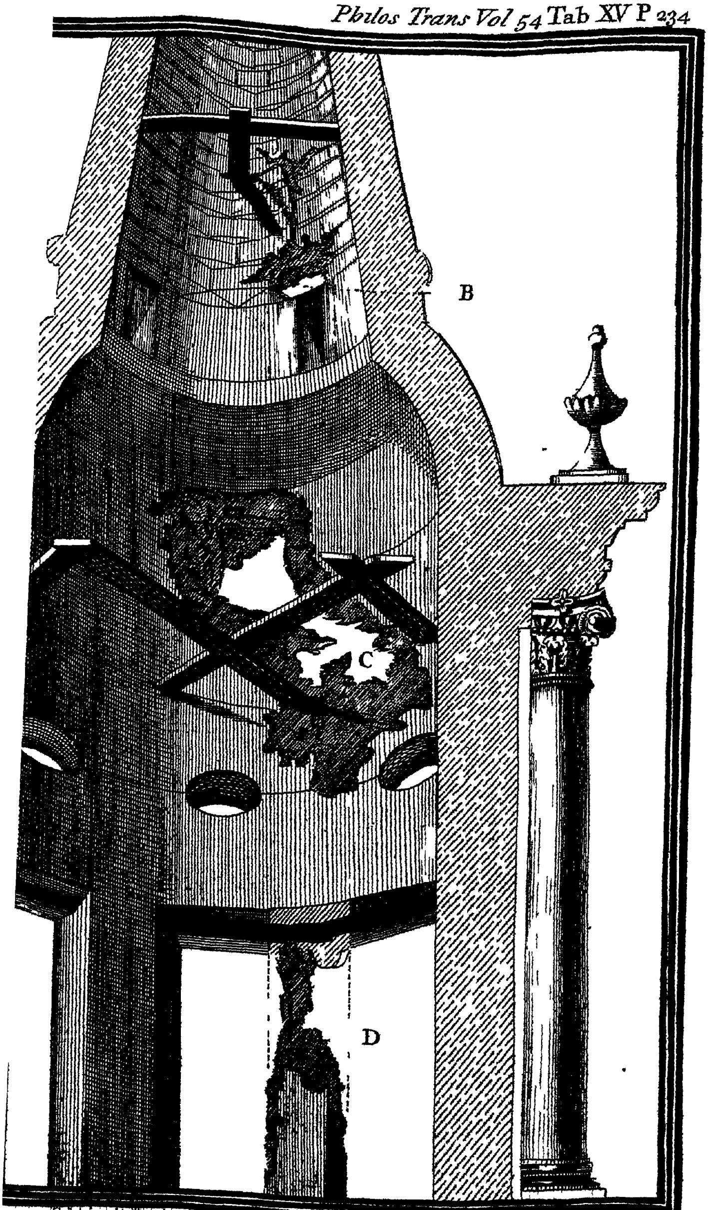
C



B

Fig.1.




B
C
D

XLII. *A Letter from* Thomas Lawrence, *M. D. to* William Heberden, *M. D. and F. R. S. concerning the Effects of Lightning, in* Eſſex-ſtreet, *on the* 18*th of* June, 1764.

S I R,

Read July 5, 1764.

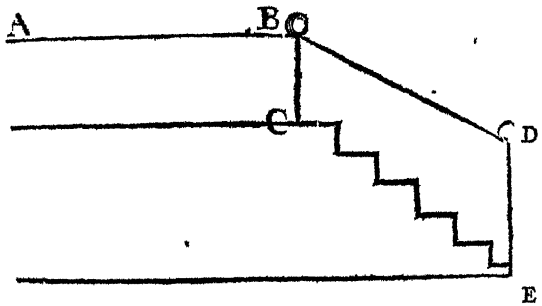
I Send you, as you deſired, an account of the effects the lightning on Monday ſe'nnight had in my neighbourhood. The ſtorm, which came from the South-eaſt, broke firſt on the two houſes at the bottom of Eſſex-ſtreet (which look from their ſouth windows upon the river) and beat down ſeveral feet of the eaſt-flue of the chimney on the weſt ſide, and ſeparated the remainder down to the roof of the houſe from the weſtern flue by a wide crack. From hence the lightning went higher up the ſtreet, and at the diſtance of about eighteen yards from the chimney juſt mentioned, went thro' the eves of a houſe, in a direction from the North-eaſt to the Southweſt, as appeared by the breach, and forced the cieling of the garret inward by a kind of pointed bulge, without breaking the laths. It continued up the ſtreet, perhaps along the leaden gutter, over the eves of the houſes for thirty yards, as I gueſs, and turned downward by the ſide of a leaden pipe made to convey the water from the top of the houſe, and tore a wooden caſe at the lower end of that pipe, cracked the wall near that place, and broke ſeveral

panes of glaſs in the kitchen window next it. The wall that was cracked was blackened, and there was a ſtrong ſmell of ſulphur in the ſtreet.

On the caſt ſide of the ſtreet, the lightning broke the ſouth garret window of the bottom houſe, threw down the eaſtern flue of the chimney down to the roof of the houſe, and took away part of the weſtern flue. The lightning ſeemed to have paſſed between the garret window and the chimney, as the window was damaged on the weſt ſide; but the chimney, which ſtood weſt of the window, on the eaſt ſide.

The tiles on the roof of both houſes were broken both on the ſouth and north ſide in a deep ſurrow, as if a heavy plough had paſſed over them.

The houſe laſt mentioned has a door on the eaſt ſide, which opens into a garden looking into the Temple; from this door there are ſeveral ſtone ſteps down to the garden. On the left hand of the ſteps is an iron rail. I have repreſented the ſteps and rail as well as I can in the figure.



A B is an iron rail ſupported by an iron baluſter B C; B D is the ſame rail continued down the ſide of the ſteps, and ſupported at D by the iron baluſter D E. The lightning, conducted (as I ſuppoſe) by the

the rail AB to B, and from thence by the baluster BC to C, ſtruck off the corner of the ſtone ſtep at C, without any diſcolouring of the ſtep; the piece ſtruck off might be three or four pound's weight. Part of the lightning, conducted farther from B to D along the iron rail was carried by the baluſter DE to E, and a large piece was ſtruck off from the corner E of the ſtone ſtep; there was no diſcolouring of the ſtep. The piece, which I took up in my hand, might be three or four pounds weight, and fitted the broken corner of the ſtep exactly. This iron rail is within three feet of a leaden pipe, which comes down from the top of the houſe, and is not continued to the ground.

The lightning went up the eaſt ſide of the ſtreet without any effect, till, at about the diſtance of 70 yards from the bottom houſe, it ſtruck the flag pavement near the iron rails of the adjoining houſe, and broke off a piece of the flag ſtone, weighing, perhaps, two pounds; there was no diſcolouring here, but, as in the ſtone ſteps before mentioned, the appearance was as if the ſtone had been broken by the blow of a ſledge hammer. One continued leaden gutter runs over the eves of theſe houſes on the eaſt ſide as well as on the weſt ſide.

The effects of the ſhock were very particular on ſome perſons. A lady in the bottom houſe on the eaſt ſide, who had left the room which looks over the river, to avoid the lightning, and ſat near a window which looks directly up the ſtreet towards the north, fell from her chair; but her ſurprize was ſo great that ſhe cannot ſay whether ſhe was thrown down by the concuſſion of the air, or fell by the fright.

fright. She ſays, ſhe felt the lightning on her arm, and had a very odd ſenſation like what ſhe ſuppoſes people feel by the electrical ſhock; ſhe further ſays, her arm ſmelt very ſtrong of ſulphur for a conſiderable time, though ſhe went out of the houſe immediately.

Another lady, who lives on the weſt ſide of the ſtreet, in the houſe the roof of which was bulged in, as has been mentioned, as ſhe ſat on the bed with a window open behind her, which looks to the weſt, was thrown off the bed on a child, who ſat on a chair by the bedſide. The ſenſation the ſhock gave her, was as it were of a blow croſs her ſhoulders.

My houſe is on the eaſt ſide of the ſtreet, next door but one to that where the ſteps were broken and the chimney thrown down. I was at home in the fore room on the ground floor. I felt a greater ſhock and concuſſion in the air than I had ever obſerved before from thunder. A gentleman, who was with me, ſays, what he felt was moſt like the ſenſation produced by the preſſure of the water when a man leaps into it.

I am, Sir,

With great reſpect,

Your moſt humble ſervant,

Eſſex-ſtreet,
June 28, 1764.

Thomas Lawrence.

XLIII. *An*

XLIII. *An Account of what appeared on Opening the Body of an asthmatic Person: By* W. Watson, *M. D. F. R. S.*

To the Royal Society.

Gentlemen,

Read July 12, 1764.

AS nothing tends more to illustrate the nature of uncommon diseases than the examination of morbid bodies after death, I take the liberty of communicating the following history.

Mr. W. aged twenty eight, consulted me at the latter end of November 1763. He had for about two months laboured under great difficulty of respiration, for which he had taken great variety of medicines without any relief. He had been largely bled, which, without helping his respiration, had greatly depressed and weakened him. The air of the country, where he had resided some time, made not the least alteration in his complaint, which daily grew worse.

When I saw him, his breathing was exceedingly difficult; the heat of his flesh moderate; his pulse too quick, beating about an hundred strokes in a minute, and withall low and thready, indicating great want of fluid in the arterial system. He coughed very frequently, and what he expectorated was viscid; but neither purulent nor bloody, and in no great quantity. He was without thirst. His nights were almost without sleep, on account of the difficulty

culty of refpiration; he not having been able to lie down in bed for fome time, more efpecially on his right fide.

To relieve him, I ordered at intervals bliſters to his legs and fide, which, though they difcharged liberally, did fcarce help his refpiration. I likewife during about ten days, that I faw him, directed folutions of the fœtid gums, volatiles, *Conf. Damocratis cum Vino Antimon.* Vinegar of fquills, volatile oily mixtures, and other antifpafmodics and attenuants with plentiful dilution. The *Confect. Damocratis cum Vino Antimoniali* at firſt much relieved him, and he was enabled to lie down in his bed; but this relief was temporary.

He generally grew hot, and more uneafy towards evening; and one night, during my attending him, he was feized with a violent pain in his right fide. For this he loſt feven ounces of blood, which, though it fomewhat funk him, very much abated the violence of his pain.

Frequently likewife in the night, notwithſtanding that the feafon was cold, he, on account of the difficulty of his refpiration, ordered and infiſted upon the windows of his chamber being opened, to gratify himfelf with yet cooler air than that of his chamber.

Two or three days before he died, his refpiration was extreamly laborious; he fweated profufely; and his ſtrength growing lefs and lefs, he expired without the leaſt convulfion.

Several days before his death, he took large dofes of Vinegar of fquills four or five times a day. This, though it did not in the leaſt offend his ſtomach, did not

not promote his expectoration, which ſtill continued viſcid.

From a careful conſideration of this diſeaſe, I was of opinion, that it was confined to the lungs: that theſe, eſpecially on the right ſide, adhered to the pleura: that their ſubſtance was occupied by tubercles, or ſomething analogous thereto, which greatly diſturbed their functions. The feveriſh heat and quick pulſe I conſidered as ſymptomatic of, and occaſioned by, his extremely laborious reſpiration.

As I was very deſirous of ſeeing the ſtate of his lungs after death, my requeſt to ſatisfy myſelf was complied with; and this examination was ſufficiently convincing, that the diſeaſe was of too ſevere a kind to admit of a cure.

Upon lifting up the ſternum, the lungs were enormouſly diſtended with air, which no preſſure could force back through the windpipe. This air was extravaſate, had burſt through the extremities of the *bronchia* and veſicular ſubſtance, and had inſinuated itſelf throughout the whole ſubſtance of the lungs, in which it was detained by the membrane inveſting them. In a word, the whole ſubſtance of the lungs was in a ſtate truly emphyſematous. In ſeveral parts this air had formed large bladders, which, though no preſſure upon the ſurface of the lungs could force back, a ſlight inciſion into them permitted to eſcape, and cauſed the whole lobe to collapſe.

Beſides this emphyſematous affection of the whole ſubſtance of the lungs, the pulmonary vein was in all its parts diſtended into numberleſs *varices*, many of

of which were of the ſize of the ſmall, or Lucca olive, and were diſtended with grumous blood. Beſides theſe, there was a larger ciſt in the right lobe of the lungs, which was filled with deep-coloured ichor: this lobe adhered to the pleura in great part of its ſurface. The lungs in general were of a deep red colour, and here and there upon their ſurface beginning to ſphacelate.

The figure of the human heart is that of a cone, divided through its axis; but in this caſe, the heart's figure was altered, and was more compreſſed than uſual; and its ventricles diſtended with grumous blood.

Every other part of the body was in its natural ſtate.

From this examination we find that in this inſtance reſpiration was greatly diſturbed, to ſay nothing of the ciſt in the right lobe, nor of the adheſion of that lobe to the pleura, from two manifeſt and potent cauſes; viz. the varicoſe ſtate of the pulmonary vein, and the emphyſema throughout the whole ſubſtance of the lungs.

The *varices* of the pulmonary vein not only retarded the blood in its paſſage to the left auricle of the heart; but, occupying a much larger ſpace in the lungs than they naturally ſhould, they left leſs room for the minute ramifications of the bronchia to extend themſelves; and conſequently a leſs quantity of air was taken in at every inſpiration than was neceſſary for the ordinary purpoſes of life.

But the diſorder of the lungs from the *varices* was made infinitely worſe by the emphyſema. For by the extravaſate air poſſeſſing ſo large a portion of the lungs, and which the patient could by no means part with

with in exſpiration, very little room was left for freſh air in inſpiration; the lungs, from the emphyſema, and from the diſeaſed ſtate of the pulmonary vein, filling almoſt the whole cavity of the thorax. This not only occaſioned an enormous defect in the quantity of air in inſpiration neceſſary to the purpoſes of life, but by the preternatural compreſſion the motion of the blood was retarded in the lungs, more eſpecially in their ſmaller veſſels. This affected not only the ſerous extravaſation in the ciſt before-mentioned, but occaſioned thoſe general obſtructions in the blood veſſels of the lungs, which brought on the ſphacelated appearance; and finally, by the increaſe of the complaint, was the cauſe of death.

This extraordinary diſtenſion of the lungs accounted for the heart's being of a more compreſſed figure than is uſually ſeen.

In the preſent inſtance an aſthma was occaſioned by two cauſes, either of which has hitherto been ſcarce conſidered as conducing thereto; the one an emphyſema, and the other a varicoſe affection of the pulmonary vein. Had the cauſes of this diſeaſe been as perfectly known during the life of the patient, as ſince his death, the caſe would not have admitted of a cure; as there was no method of diſcharging the extravaſate air from the lungs; neither could any medical proceſs alter or amend the varicoſe ſtate of the pulmonary vein.

Such a ſtate of lungs, as that juſt now deſcribed, in an otherwiſe healthy young man, could not, I was perſuaded, happen but from ſome very powerful cauſe; and, upon enquiry, I was informed, that about the beginning of October, not two months before his death, from ſomething which had greatly offended

his ſtomach, he was ſeized with violent and long continued vomitings. Theſe, though at length they were quieted, left his cheſt very ſore. From this time his cough became troubleſome, as did remarkably his ſhortneſs of breath upon the leaſt motion, attended with the ſeveral circumſtances above deſcribed.

From conſidering the hiſtory of this diſeaſe, and comparing it with the appearance of the lungs after death, I cannot but be of opinion, that the violent efforts to vomit occaſioned primarily both the emphyſema, and the *varices* of the pulmonary vein. This opinion, I flatter myſelf, will not, to perſons well verſed in the animal œconomy, ſeem ill founded, when they reflect how forcibly the lungs are preſſed in violent efforts to vomit, both by the muſcles ſubſervient to reſpiration and the abdominal muſcles, as well as by the contents of the abdomen itſelf. And it is wonderful, when the texture of the lungs is conſidered, that accidents of this kind do not much oftener happen, not only in vehement reachings to vomit, but in violent coughs, pains of childbirth, lifting great weights, and other preternatural exertions of ſtrength.

When once the extremities of the bronchia and the veſicular ſubſtance have given way, the miſchiefs are eaſily foreſeen. The air getting looſe into the ſubſtance of the lungs cannot be parted with in exſpiration; it conſequently is retained there, and the ſpace it occupies prevents as much of the external air being received into the lungs as its own quantity. As, from their inceſſant motion, injuries to the lungs are not eaſily removed, when once a rupture is made, every fit of coughing or other violent exertion extravaſates more air. Hence the rup-

ture

ture ſtill continuing, and probably increaſing, more and more air becomes extravaſate, until, as in the preſent caſe, the quantity becomes ſo great, as not only to impede the courſe of the blood through the lungs, but the internal preſſure of the extravaſate air prevents the ingreſs of a quantity of freſh air, ſufficient to cool and attenuate the blood. In fact, a ſmall part only of the lungs is employed; as the extravaſate air, though ſtill in an elaſtic ſtate, anſwers by no means the purpoſes of freſh air in reſpiration; as the former, by its confinement in the lungs, is very ſoon deveſted of its vivifying ſpirit, that principle which is ſoon deſtroyed in animal bodies, and which ſome chemical phyſiologiſts have ſuppoſed to be an acid nitrous *Gas*, and is moſt eſſential to human life. Hence, in a very ſhort time, the effects are too obvious to be mentioned; and death muſt ſoon follow, as happened to the perſon, who is the ſubject of this communication.

I am, with all poſſible regard,

Gentlemen,

July 6, 1764. Your moſt obedient

Humble ſervant,

W. Watſon.

XLIV. *An*

XLIV. *A Letter to the Marquiſs of* Rockingham, *with ſome Obſervations on the Effects of Lightening.*

To the moſt honourable the Marquiſs of Rockingham.

My Lord,

Read July 28, 1764.

I Have taken the liberty of addreſſing the incloſed paper to your Lordſhip, as it contains a few obſervations on a ſubject not unknown to you, and may probably be thought of general uſe.

I have a further reaſon for addreſſing it to your Lordſhip, as it gives me an opportunity of expreſſing, in ſome ſmall degree, the high ſenſe of gratitude I bear your Lordſhip, for many and repeated favours and obligations conferred on,

My Lord,

Your Lordſhip's moſt obedient,

and moſt humble ſervant,

June 24, 1764,
Great Queen-ſtreet,
London.

Benjamin Wilſon.

LONG

Confiderations to prevent Lightening from doing Mifchief to great Works, high Buildings, and large Magazines: By Mr. Wilfon, *F. R. S. and Member of the Royal Academy of Sciences at* Upfal.

LONG experience, fince the difcovery by Dr. Franklin, has now eftablifhed a truth amongft philofophers, that lightening, like the electric fluid, paffes more freely through iron, copper, and other metals, than through dry wood, ftone, or marble.

Inftances of this truth are innumerable: and to convince us thereof, we need only trace the late violent effects of lightening on St. Bride's Church, and the houfes in Effex-ftreet, &c.

For, upon examining thefe buildings, it appears, that there are certain thick bars of iron, through which the lightening has paft, without producing any vifible effects: and on the contrary, in certain parts where the junctions of thofe bars with the ftone, or wood, are made, there the lightening, rufhing from the iron, has broke the ftone to pieces, and fhivered the wood.

From the like experience we alfo learn, that if the iron is too flender for conducting the lightening, it is either dafhed into pieces, or exploded like gunpowder; juft in the fame manner as we are able, by the electric power, to break and diffipate in vapour a very flender wire. Bars of metal, of a proper thicknefs, and conveniently difpofed, feem therefore neceffary for the fecurity of fuch buildings.

It is to be noted, that the mifchiefs caufed by lightening are not always owing to its direction from the clouds to the buildings or other eminences, and thence to

to the earth; but ſometimes, on the contrary, from the earth, buildings, and other eminences, to the clouds. For the principle upon which its direction depends, appears to ariſe from the reſtoration of a certain *equilibrium*, in a ſubtile and elaſtic fluid, previouſly diſturbed by various cauſes.

Now, according to the laws of elaſtic fluids, the endeavour to reſtore the *equilibrium* of ſuch a fluid, will be in that direction, where the *reſiſtance* to it's paſſage happens to be the leaſt. Upon this principle we therefore ſee a neceſſity, either to open a paſſage for it to go freely through, by placing certain bars of metal properly; or, to ſtop the paſſage of the fluid through ſuch buildings entirely.

The laſt method would be dangerous to put in practice; becauſe, if high buildings were ſo ſecured, the lightening would then attack the lower buildings, which are far more numerous, and probably would deſtroy a greater number of people, cattle, &c.

Whereas, if the firſt method is preferred, the high buildings will then tend to protect the lower ones more effectually; and may with propriety be conſidered, as ſo many pipes to carry off the lightening quietly; either from the earth to the clouds, or from the clouds to the earth.

And that ſeveral proper conductors are neceſſary to carry off the lightning more readily, than ſome of the *accidental* or *partial* conductors, in a large town, are capable of, appears from this; that we are able to collect ſmall quantities of the electric fluid, with a ſlender apparatus in our hands only; whilſt it is expoſed in the ſtreet, garden, or other open place, during the hovering of ſuch clouds as occaſion violent lightening.

4 From

From repeated obſervations of this kind, there is reaſon to believe, the quantity of lightning at particular times is ſo very great, that it would be dangerous to invite it to any buildings, and that unneceſſarily, in the moſt powerful manner we are able; by ſuffering the ſeveral conductors to end in a *point* at the top.

On which account, it is apprehended, *pointed bars*, or *rods of metal*, ought always to be avoided.

And as the lightening muſt viſit us, ſome way or other, from neceſſity, to reſtore the *equilibrium*, there can be no reaſon to invite it at all: but, on the contrary, when it happens to attack our buildings, we ought only ſo to contrive our apparatus, as to be able to carry the lightening away again by ſuch ſuitable conductors, properly fixed, as will very little, if at all, promote any encreaſe of it's quantity.

To attain which deſirable end, in ſome degree at leaſt, it is propoſed, that the ſeveral buildings remain as they are at the top; that is, *without having any metal above them, either pointed or not*, by way of a conductor.

On the inſide of the higheſt part of ſuch building, and within a foot or two of the top, it may be proper to fix a rounded bar of metal, and to continue it down along the ſide of the wall to any kind of moiſture in the ground.

But if the building happens to be mounted with an iron ſpindle, for ſupporting a vane, or other ornament, and it ſhould not be convenient to have it taken away, then the bar of metal ought to communicate with that ſpindle.

And in regard to the diameter of ſuch a metal bar, it will probably depend upon the height of

of the building: for it is apprehended the great church of St. Paul's, to compleat the partial conductors (which are the metallic croſs, ball, gallery, dome, &c.) and ſecure it effectually, would require a bar of metal two inches diameter, if not more: and a building like the British Muſeum, one conſiderably leſs. But it appears there is no occaſion for any at that repoſitory, as it is already provided, though from *accident*, like many other buildings, with very effectual conductors. The copeings of the roof thereof, and the ſeveral ſpouts, which are continued from thence into the ground, being all of lead.

That conductors ought to be thicker than is generally imagined, ſeems to appear from a late inſtance taken notice of in St. Bride's church by Mr. Delaval and Dr. Watſon, where an iron bar two inches and a half broad, and half an inch thick, or more, was bent and broke aſunder by the violence of the lightening.

The Eddyſtone Lighthouſe, which ſtands upon a rock ſurrounded by the ſea, the work of Mr. Smeaton, was thought to be an object very likely to ſuffer by lightening; and the more ſo, as the top of it conſiſted of a copper ball two feet in diameter, with a chimney of the ſame metal, paſſing through it down to the ſecond floor, but no further. Directions were therefore given to make a communication of metal from the loweſt part of the copper chimney down to the ſea; which was executed accordingly about the year 1760, or ſoon after the building was finiſhed. Now if, inſtead of the copper ball, a pointed bar of metal had been put in its place, or above it, and communicated with the conducting matter below, there is no ſaying what might be the conſequence of

ſo

ſo powerful an invitation, to an edifice thus particularly ſituated.

Read Nov. 8, 1764.

SINCE the former part of this paper was communicated to the Royal Society, that is, on the 5th of Auguſt, 1764, I received the following account from captain Dibden, commander of a merchant ſhip, who ſays, that in the year 1759, he was taken by the French, and carried priſoner to Fort Royal in Martinico. That in removing him from thence ſome time after, and on foot to St. Pierre, which is about 20 miles, his conductor, or guard, ſtopped at a ſmall chapel five miles from the laſt place, to ſhelter themſelves from the heavy rain which fell during a violent thunder ſtorm. That the chapel had no ſteeple or tower belonging to it, but ſtood upon an eminence with three or four poor low houſes near it. That ſoon after they were thus ſheltered, a violent flaſh of lightening ſtruck two ſoldiers dead, who had been leaning againſt the wall of the chapel between two buttreſſes, and not far from the reſt of the company; they being all on the leeward ſide of the chapel.

That it made an opening in the wall about four feet high, and about three feet broad, and in that part only againſt which they reſted.

That captain Dibden, along with other perſons, entered at this hole immediately after, to ſee if any other damage had been done to the chapel. That they obſerved a ſquare bar of iron near the hole, and upon the ground, about four feet long, and *one inch and a quarter thick*, making an angle with the wall, as they ſuppoſed, to ſupport the upper part of an inclined tombſtone, which was alſo thrown down and

broke

broke to pieces. That this bar was joined in the middle to one end of another bar, about one foot long, and *one inch thick*, which laid horizontally, and, passing to the wall, had been there fastened with lead. That the lightening in rushing along the inclined bar, had wasted or reduced its thickness in some places very considerably: insomuch, that it looked like a burnt po' er which had been long used : and broke the bar into two pieces, about an inch above the joining of the lesser bar; the ends of which had a burnt flaky appearance. That the other parts of the bar were changed in colour to a grey, or whitish hue; resembling iron, after it has been exposed to a violent heat, and then suffered to cool.

That the horizontal bar had also undergone an extraordinary change by the lightening, but particularly at that end next the wall of the chapel, it being reduced from one inch in diameter, to the size of a slender wire, but *tapering towards the wall*.

That when the soldiers rested against the wall, their heads were about the same height with the shortest bar; and, from what he can recollect, were very near being opposite to that end thereof, which was inserted in the wall.

That the two soldiers were forced from the wall at the same instant by the lightening: so that their feet, which were one yard or more from it, were nearest to the wall, and their heads the farthest off. That their flesh appeared very black. That their cloaths were burnt and scorched in many parts: and their belts shriveled up, as if they had been exposed to a large fire. That captain Dibden, and other people, felt a disagreeable kind of an electric shock, at the same instant that the soldiers were killed.

Captain

Captain Dibden gave an account alſo, that he was lately at Virginia, 1763: that the inhabitants of Norfolk had changed their opinions in reſpect to fixing of wires and ſmall rods of iron on the tops of their houſes; from the frequent inſtances they have lately had of their being melted, or deſtroyed, by the violence of the lightening: and that now they adopted in their ſtead, rods of iron from half an inch thick, to three quarters of an inch thick, or more. That thoſe rods ended in a point at the top, and extended from three feet above their houſes down to the ground: and that many houſes had one of theſe conducting irons at each end.

This account appears very material upon the preſent occaſion, as it ſerves to confirm the conjectures that are now offered, in a manner ſo obvious as to require no particular explanation.

The captain added, that, though the pine trees are conſiderably higher than the oaks in the American woods, yet the oaks are the ofteneſt attacked by the lightening: and that he does not remember any oaks growing among the pine trees, when the latter have ſuffered by lightening: which muſt be owing to the greater *reſiſtance* ariſing from the unctuous nature of the pine trees.

XLV. Solis *Defectus observatus in Collegio* Romano *a Patribus Societatis* Jesu *die prima* Aprilis *Anno* MDCCLXIV. *Tempore vero post mediam noctem.*

Read Nov. 8, 1764.

IN hoc observando Solari deliquio adhibuimus præcipue telescopium objectivo micrometro instructum, egregium opus Jacobi Shortii, a Prænestino principe iterum jam humanissime nobis commodatum, pro quo beneficio maximam illi, dum erimus, habebimus gratiam. Cum hoc instrumenti genere in Italia nulla sit hactenus observata eclipsis, ab exteris vero nationibus, si quid hac in re præstiterunt, nihil ad nos pervenerit, quod & aliis pluribus contigisse arbitrari fas est, observationem hanc nostram, astronomis præsertim Italis, non injucundam fore censuimus.

Dissipata nebula rarae nubes, quæ superfuere, impedimenti quidem aliquid observationi attulerunt, ut plurimum tamen Sole satis claro usi sumus.

Ex observationibus decem diameter Solis deficientis, cui ex Pariensi Ephemeride respondent 1921″, 4, deprehensa est intercipere micrometri particulas 2260: Summum autem inter omnes discrimen particulas duas non excessit.

Eclipsis initium animadversum est tubo palmorum decem $9^h\ 49'\ 8''$: Finis vero eodem tubo $12^h\ 52'\ 49''$.

Definivimus tum chordas obscurationis, tum distantias limborum Solis ac Lunæ secundum lineam conjun-

conjungentem utriusque centra, nulla digitorum habita ratione: maluimus enim tempus adnotare, quo aliquid accurate observatum videbatur; quam expectare, ut obscuratio certam mensuram attingeret, ne aut nubes, aut impedimenta, quæ loci angustiæ objecere plurima, sæpius observationem subriperent; nullus autem ad curiosos arcendos homines rem omnem perturbaturos opportunior locus occurrebat: eædem fuere caussæ, cur hoc instrumentum adeo accurate aliqua non exhibuerit, ut in aliis pluribus experti sumus.

Chordæ limbi obscur.	h	'	''		Dist. limb. Solis, et Lunæ.	h	'	''	
1	9	56	31	817	I.	10	6	59	1853
2	10	0	14	1016	II.		16	18	1636
3		5	0	1192	III.		33	3	1273
4	11	26	19	2095	IV.		56	38	825
5		31	17	2083	V.	11	9	49	656
6		34	11	2077	VI.		15	18	618
7		53	53	1949	VII.		21	45	618
8	12	11	53	1729	VIII.		39	32	798
9		21	32	1561	IX.		45	16	894
10		25	12	1484					
11		27	28	1432					
12		29	16	1388					
13		30	43	1349					
14		33	58	1257					

Ex observationibus VI, VII, VIII invenimus per interpolationem limborum distantias tempore observationum 4, 5, 6 ex ordine fuisse 639, 682, 716.

Ex

Ex datis eodem tempore Solis diametro, chorda partis obſcuratae, & limborum diſtantia, in ſingulis obſervationibus Lunæ diameter in micrometri particulis ex ordine prodiit 2114, 2110, 2114. Hinc Lunæ diameter circa horam 11 30′ ſtatui poteſt partium 2112,7, quibus reſpondent ſcrupula ſecunda 1796,1. In inveſtigandis tamen centrorum diſtantiis, quas in ſequenti tabula in ſcrupulis ſecundis damus, uſi ſumus conſtanti diametro 1795″, quia totum inter maximam & minimam diſcrimen Eclipſis tempore eſt circiter 3″, & ea, quam mox invenimus, media eſt aliquanto major.

DISTANTIÆ CENTRORUM.

Ex obſ.	h	′	″	″		h	′	″	″
1	9	56	31	1723	6	11	34	11	540
2	10	0	14	1645	VIII.		39	32	615
3		5	0	1557	IX.		45	16	697
I.		6	59	1512	7		53	53	832
II.		16	18	1328	8	12	11	53	1134
III.		33	3	1019	9		21	32	1299
IV.		56	38	638	10		25	12	1363
V.	11	9	49	495	11		27	28	1403
VI.		15	18	462	12		29	16	1435
VII.		21	45	462	13		30	43	1461
4		26	19	472	14		33	58	1520
5		31	17	519					

Ex obſervationibus IV, VI, VII, VIII, mutuo comparatis minima centrorum diſtantia contigiſſe videtur 11^h 18′ 45″, eaque fuiſſe 458″, adeoque maxima obſcuratio digitorum 8 45′. Juniores aliqui ex noſtris ex Solis imagine per tubum opticum in chartam tranſmiſſa obſcurationem maximam, nobili & erudita

ſpectante

ſpectante multitudine, æſtimarunt digitorum fere $8\frac{3}{4}$, alius vero alibi eadem ratione obſervans digitorum $8\frac{1}{3}$, ac pluribus habitis circa illam obſervationibus vix $10'$ in tempore definiendo a noſtra determinatione defecit.

Lunæ diametrum ex eclipſi definitam cum diametro extra Solem conferre conati ſumus: verum die tertia Aprilis ægre admodum obſervari potuit propter nimiam Lunaris lucis tenuitatem, cui plurimum etiam officiebant reſiduum aliquod crepuſculi lumen, & in horizontis vicinia vapores. Eas proinde obſervationes, licet multum neque inter ſe, neque ab inventa diametro diſcreparent, rejecimus, ut certiores obtineremus die quarta; ſed ea quoque die non levem experti ſumus difficultatem, quia Lunæ cornua in exiliſſimas tenuabantur lineas ab ejuſdem aſperitatibus umbra identidem interruptas: eas nihilominus, ut nobis ſeſe obtulerunt, hic ſubjicimus in micrometri particulis.

DIE QUARTA APRILIS.

Obſ.	h	′		Diam. Lunæ.
1	8	0	Veſp.	2096
2		7		2094
3		17		2093
4		23		2087
5		29		2088
6		35		2086

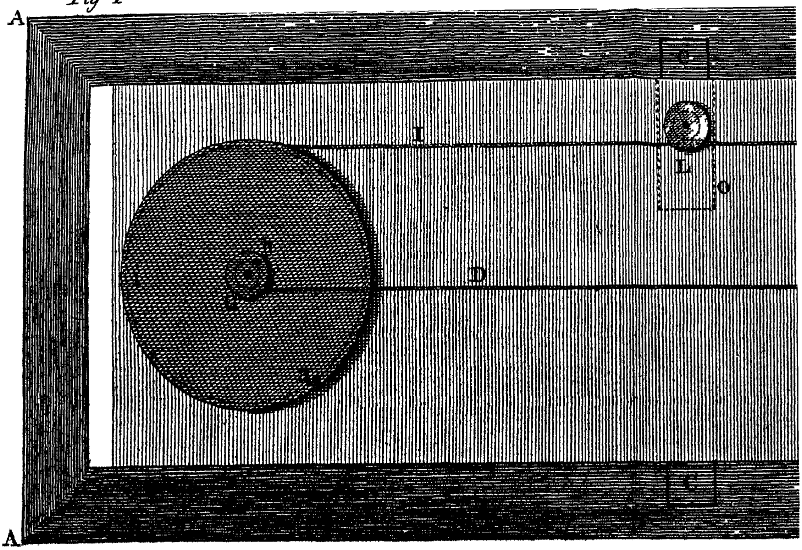
Converſis micrometri particulis in ſcrupula ſecunda, primo correximus diametri decrementum a refractione ortum, nam diameter obſervata inclinabatur ad horizontem gradibus circiter 18, ut idem teleſcopium indicavit: hæc autem correctio in ultima obſervatione,

in qua maxima fuit, inventa eſt 1″,2. Secundo, ex obſervationis hora determinavimus Lunæ horarium, ac declinationem, atque inde ejus altitudinem tum veram, tum apparentem. Tertio, ex iis inveſtigavimus Lunæ diametrum horizontalem ope trianguli conſtituti a ſemidiametro terreſtri, atque a rectis ductis a centro Lunæ ad ſpectatorem, & ad telluris centrum. Ex ſingulis obſervationibus hæ prodierunt.

		Diam. horiz. Lunæ.
		″
Ex. obſ.	1	1773,7
	2	1772,8
	3	1772,9
	4	1768,4
	5	1770,0
	6	1769,0
	Media	1771,1

Diameter Lunæ horizontalis die quarta Aprilis circa horam octavam ex Pariſ. Ephem. eadem eſt adamuſſim, ac in ipſa eclipſi, in quo ſenſibilis error eſſe non poteſt; cum enim Luna circa Apogæum verſaretur, diametri variationes erant exiguæ, quas Aſtronomicæ Tabulæ exhibent ſatis accurate: tantum augenda eſt ea quantitate, quam poſtulat apparens altitudo Lunæ 11^h 30′, quæ fuit circiter grad. 52$\frac{1}{2}$: quo facto additamento diametrum Lunæ obtinuimus 1793″,2. Ab ea quam ex Eclipſi collegimus, diſcrimen eſt 3″, quod ad unicum redigitur, ſi rejiciantur tres poſteriores obſervationes, quas minus certas arbitramur, quia oculus prioribus defatigatus (ſumma enim opus erat contentione) fortaſſe factus eſt minus idoneus ad languidam Lunæ lucem percipiendam.

Fig 1


A
A
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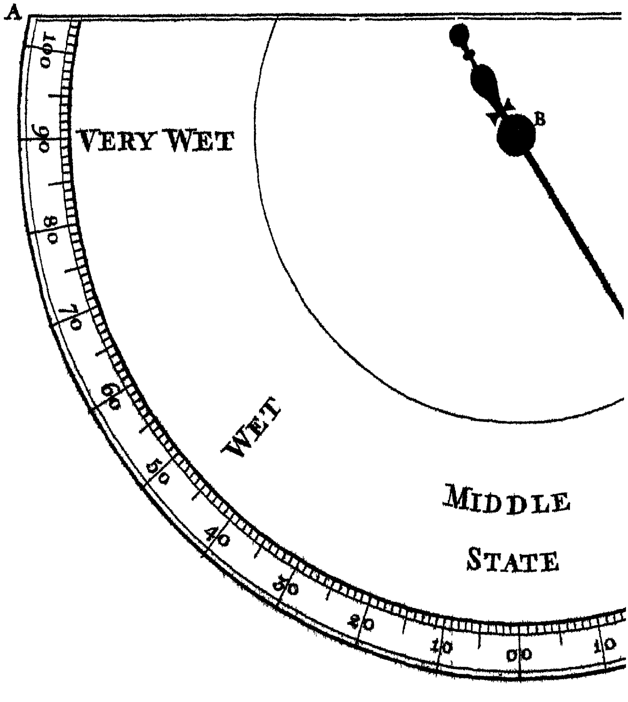
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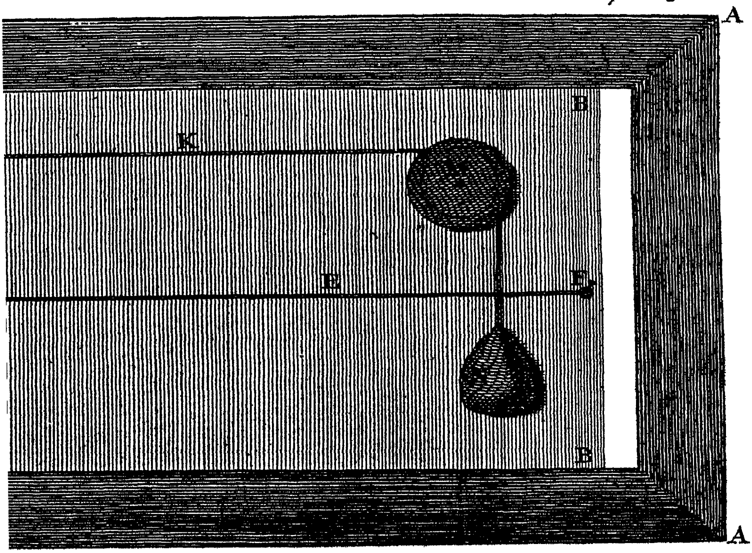


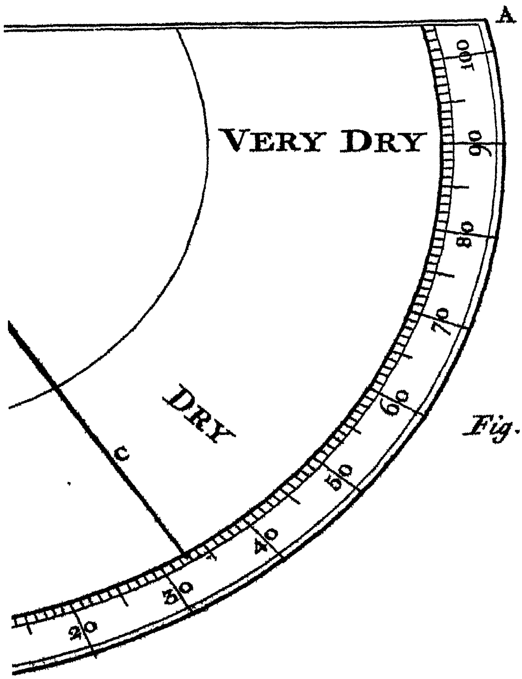


Fig. 2.

XLVI. *The Description of a New Hygrometer, invented by* James Ferguson, *F.R.S.*

Read Nov. 8, 1764.

IN this machine (Tab. XVI. Fig. 1. and 2.) AAAA is a frame of wainscot or mahogany, grooved in the innermost edges of the two longest sides, for holding a pannel BBBB of white deal-board, without pinching it. The pannel is about the thickness of a crown piece, and fifteen inches in length, cross-wise to the grain of the wood. The middle part projects outward from the upper and lower edges, at C and C, where it is fastened into the frame by two screws, to keep the middle part always in the same place, whilst the rest of the pannel expands by moist air toward both ends of the frame, and contracts toward the middle when the air is dry. F is a pin fixt into the pannel near one of its ends, and a round pin is fixt near the other end of the pannel, on which the large pulley H turns, and also the small pully G which is fixed to H. One end of a small flexible cord DE is fastened to the pin F, and the other end goes round the pully G, and is fixt into the bottom of its groove, as at *b*. One end of another small cord IK is fixed into the bottom of the groove of the large pulley H, as at *a*, from which it goes round the part *a i* H; and in its way thence to M it goes round a small pulley L, in which an axis is fixt, and turns in the piece O, which lies above the pulley, and is screwed to the upper side of the frame at C. This cord goes over the pulley M (which turns on a round pin fixt into the pannel) and has a flattish weight N hung to it.—The pullies G and L are of equal diameters in their grooves, which is only equal to a tenth part of the diameter of the large pulley H in its groove. The pulley M may be of any convenient size.

Now it is plain, that as much as the pannel expands between F and G, so much will the pully G be removed farther from the pin F; and just so much will the cord DE turn the pulley G backward; and any point in the groove

groove of the pulley H ten times as much, becauſe it is ten times the diameter of G in the groove: and this motion will cauſe the cord I K to turn the pulley L (and draw up the weight N) ten times as much as the pulley G is turned. So that, if the pannel expands a tenth part of an inch, by moiſt air, the pulley L will be turned quite round: and half round if the pannel expands but a 20th part of an inch: As the air grows dry, the pannel contracts, and the weight M deſcends and turns all the pullies the contrary way.

The back of the plate A A (Fig. 2.) is ſcrewed to the other ſide of the frame (Fig. 1.) ſo as the ſtraight edge of the plate may be even with the uppermoſt ſide of the frame, and the center B (Fig. 2.) may be directly over the center of the pully L (Fig. 1.) on whoſe axis the index B C (Fig. 2.) is fixed. And as the pulley L is turned by the cord I K, the index will be moved on the plate, and ſhew the degrees of moiſture or dryneſs of the air.

If the expanſion and contraction of the pannel be ſo great as to move the index beyond the limits of the degrees on the plate, this may be remedied by putting on a larger pulley at L. — But if not great enough, in very wet and very dry weather, to move the index through all the degrees on the plate, the pulley L muſt be made leſs in diameter accordingly.

N. B. In three or four years at moſt, a new pannel ſhould be put into the frame: becauſe, when the old one has been ſo long expoſed to the air, it will almoſt ceaſe to be affected thereby. And therefore, a large thick piece of deal ſhould be kept in reſerve for that purpoſe; and about the thickneſs of a card always planed off that ſide from which the new pannel is to be taken.

At G and M, there muſt be ſmall knobs of ſome hard wood glued on the back of the pannel below the graduated plate, to make a proper thickneſs for holding the wires upright and faſt on which the pullies G and M do turn: for otherwiſe, the wires would ſoon looſen in the pannel.

XLVII. *Experiments and Observations on the Compressibility of Water and some other Fluids, by* John Canton, *M. A. and F. R. S.*

Read Nov. 8, 1764.

IN a paper lately laid before the Royal Society *, I not only related the experiments by which I found water to be compressible, but also those by which I discovered how much a given weight would compress it when in a temperate degree of heat. By similar experiments made since, it appears that water has the remarkable property of being more compressible in winter than in summer; which is contrary to what I have observed both in spirit of wine and oil of olives: these fluids are (as one would expect water to be) more compressible when expanded by heat, and less so when contracted by cold. Water and spirit of wine I have several times examined, both by the air-pump and condenser, in opposite seasons of the year: and, when Fahrenheit's thermometer has been at 34 degrees, I have found the water to be compressed by the mean weight of the atmosphere 49 parts in a million of its whole bulk, and the spirit of wine 60 parts; but when the thermometer has been at 64 degrees, the same weight would compress the water no more than 44 parts in a million, and the spirit of wine no less than 71 of the same parts. In making these experiments, the glass ball containing the fluid to be compressed must be kept under water, that the heat of it may not be altered during the operation.

The compression by the weight of the atmosphere, and the specific gravity of each of the following fluids, (which are all that I have yet tried,) were found when the barometer was at $29\frac{1}{2}$ inches, and the thermometer at 50 degrees.

* See Philosophical Transactions, Vol. LII. p. 640.

Com-

	Millionth parts.		Specific gravity.
Compression of spirit of wine	66	—	846
Oil of olives	48	—	918
Rain-water -	46	—	1000
Sea-water - -	40	—	1028
Mercury - -	3	—	13595

These fluids are not only compressible, but also elastic: for if the weight by which they are naturally compressed be diminished, they expand; and if that by which they are compressed in the condenser be removed, they take up the same room as at first. That this does not arise from the elasticity of any air the fluids contain, is evident; because their expansion, by removing the weight of the atmosphere, is not greater than their compression by an equal additional weight: whereas air will expand twice as much by removing half the weight of the atmosphere, as it will be compressed by adding the whole weight of the atmosphere.

It may also be worth observing, that the compressions of these fluids, by the same weight, are not in the inverse ratio of their densities or specific gravities, as might be supposed. The compression of spirit of wine, for instance, being compared with that of rain-water, is *greater* than in this proportion, and the compression of sea-water is *less*.

The weight of $32\frac{1}{2}$ feet of sea-water is equal to the mean weight of the atmosphere: and, as far as trial has yet been made, every additional weight equal to that of the atmosphere, compresses a quantity of sea-water 40 millionth parts; now if this constantly holds, the sea, where it is two miles deep, is compressed by its own weight 69 feet 2 inches; and the water at the bottom is compressed 13 parts in 1000.

XLVIII. *Concise*

XLVIII. *Concise Rules for computing the Effects of Refraction and Parallax in varying the Apparent Distance of the* Moon *from the* Sun *or a* Star; *also an easy Rule of Approximation for computing the Distance of the* Moon *from a* Star, *the Longitudes and Latitudes of both being given, with Demonstrations of the same: By the Rev.* Nevil Maskelyne, *A. M. Fellow of* Trinity College, *in the University of* Cambridge, *and F. R. S.*

Read Nov. 15, 1764.

THE following rules, excepting one, are the same which I have already communicated to the Royal Society, but without demonstration, in a letter to the reverend Dr. Birch from St. Helena, containing the results of my observations of the distance of the Moon from the Sun and fixed stars, taken in my voyage thither, for finding the longitude of the ship from time to time; since printed in Part II. Vol. LII. of the Philosophical Transactions for 1762. The two rules for the correction of refraction and parallax I have also since communicated to the public in my British Mariner's Guide to the discovery of longitude from like observations of the Moon; and have added in the Preface a rule for computing a second but smaller correction of parrallax, necessary on account of a small imperfection

imperfection lying in the first rule derived from the fluxions of a spherical triangle. To the rules I have here subjoined their demonstrations.

With respect to the usefulness of these rules, I cannot but entertain hopes that they will appear more simple and easy than any yet proposed for the same purpose: the last rule, for computing the distance of the Moon from a star, though only an approximation, being so very exact, seems particularly adapted for the construction of a nautical Ephemeris, containing the distances of the Moon from the Sun and proper fixed stars ready calculated for the purpose of finding the longitude from observations of the Moon at sea; an assistance which, in an age abounding with so many able computers, mariners need not doubt they will be provided with, as soon as they manifest a proper disposition to make use of it.

A RULE

To compute the contraction of the apparent distance of any two heavenly bodies by refraction; the zenith distances of both, and their distance from each other being given nearly.

Add together the tangents of half the sum, and half the difference of the zenith distances; their sum, abating 10 from the index, is the tangent of arc the first. To the tangent of arc the first, just found, add the co-tangent of half the distance of the stars; the sum, abating 10 from the index, is the tangent of arc the second. Then add together the tangent of double the first arc, the co-secant of double the second

Fig. 1

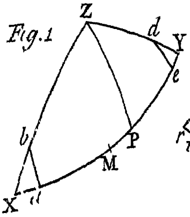


Fig. 2.

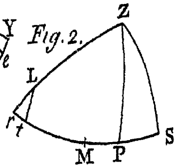


Fig. 3.

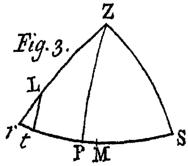


Fig. 4.

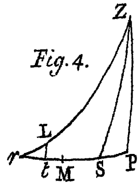


Fig. 5.

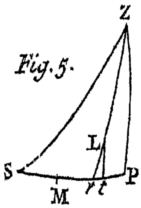


Fig. 6.

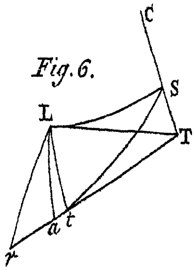
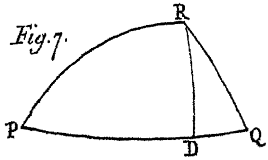


Fig. 7.



double the ſecond arch, and the conſtant logarithm of 114″ or 2,0569: the ſum abating 20 from the index, is the logarithm of the number of ſeconds required, by which the diſtance of the ſtars is contracted by refraction: which therefore added to the obſerved diſtance gives the true diſtance cleared from the effect of refraction.

Explication of the foundation of the preceding rule.

This rule is founded upon an hypotheſis that the refraction in altitude is as the tangent of the zenith diſtance: and the refraction at the altitude of 45 degrees being 57″, according to Dr. Bradley's obſervations, therefore the refraction at any altitude, calling the radius unity is = 57″ × tangent of the zenith diſtance. This rule is exact enough for the purpoſe of the calculation of the longitude from obſervations of the diſtance of the Moon from ſtars at ſea as low down as the altitude of 10°, for there the error is only 10″ from the truth. But, if the altitude of the Moon or ſtar be leſs than 10°, the rule may be ſtill made to anſwer ſufficiently, by only firſt correcting the obſerved zenith diſtances by ſubſtracting from them three times the refraction correſponding to them taken out of any common table of refraction, and making the computation with the zenith diſtances thus corrected. This correction depends upon Dr. Bradley's rule for refraction, which he found to anſwer, in a manner exactly, from the zenith quite down to the horiſon, namely that the refraction is = 57″ × tangent of the apparent zenith diſtance

leſſened

leſſened by three times the correſponding refraction taken out of any common table.

Demonſtration of the preceding rule.

Let ZXY Tab. XVII. Fig. 1. repreſent a ſpherical triangle formed by great circles joining the zenith Z and the ſtars X and Y. Refraction acting in the vertical circles ZX and ZY will carry the ſtar X nearer the zenith by a quantity $Xb = 57'' \times$ tangent of ZX, and the ſtars Y towards Z by the quantity $Yd = 57'' \times$ tangent of ZY; ſo that the apparent diſtance of the two ſtars will be bd inſtead of XY or leſs than XY, the true diſtance, by the Sun of the two little ſpaces Xa, Ye, terminated by the perpendiculars ba and de. The little ſpace $Xa = Xb \times$ coſine of the angle ZXY (calling radius unity) $=$ $57'' \times$ tang. of ZX $\times$ coſine of angle ZXY, or, by ſpherics, $= 57'' \times$ tang. of XP. (ZP being an arch drawn from Z perpendicular to the arch XY). In like manner the little ſpace $Ye = 57'' \times$ tang. of YP; and therefore $Xa + Ye$ or the total effect of refraction $= 57'' \times \overline{\text{tang. XP} + \text{tang. YP}}$. Let M be the middle of the arch XY, and put the tangent of XM or YM or $\frac{1}{2}$ XY $= t$, and the tangent of MP, or the diſtance of the perpendicular ZP from M the middle of the arch XY $= n$. By trigonometry, tang. of XP or XM + MP $= \frac{t+n}{1-tn}$, and tang. of YP or YM − MP $= \frac{t-n}{1+tn}$; the ſum of which, or tang. XP + tang. YP $= \frac{t+n}{1-tn} + \frac{t-n}{1+tn} = \frac{2t+2tn^2}{1-t^2n^2} = \frac{2tn}{1-t^2n^2} \times \frac{1+n^2}{2n} \times 2$. Now

$2tn$

$\frac{2tn}{1-t^2n^2}$ is the tangent of double the angle whoſe tangent is tn, and tn or the product of the tangents of $\frac{1}{2}$ XY and MP, by ſpherics, is equal to the product of the tangents of half the ſum and half the difference of the zenith diſtances ZX and ZY; whence $\frac{2tn}{1-t^2n^2}$ is equal to the tangent of double arch the firſt found by the rule. Alſo arch the ſecond found by the rule being by ſpherics $=$ MP, whoſe tangent is repreſented by n, and $\frac{1+n^2}{2n}$ being by trigonometry equal to the coſecant of double the arch whoſe tangent is n, therefore $\frac{1+n^2}{2n} =$ coſecant of 2 MP or double arch the ſecond. Whence the rule is manifeſt; namely, that $Xa + Ye$, the total effect of refraction in contracting the apparent diſtance of the two ſtars $= 57'' \times \overline{\text{tan. } XP + \text{tan. } YP}$ $=$ double $57''$ or $114'' \times$ tang. of double the firſt arch $\times$ coſecant of double the ſecond arch. Q.E.D.

R E M A R K.

When the perpendicular arch ZP, let fall from the Zenith on the arch XY, falls without the triangle ZXY, the effect of refraction in diminiſhing the apparent diſtance of the ſtars X, Y is the difference of Xa and Ye: but the rule being general, gives always the ſum or difference, which-ever it be, which is a great advantage, and removes all grounds of ambiguity in the correction of refraction; as the the total effect thereof always diminiſhes the diſtance of two ſtars from each other, however they are poſited.

A RULE

To compute the contraction or augmentation of the apparent diſtance of the Moon from a ſtar, on account of the Moon's parallax; the zenith diſtances of the Moon and ſtar, and their diſtance from each other being given nearly.

Add together the tangents of half the ſum, and half the difference of the zenith diſtances of the Moon and ſtar, and the cotangent of half the diſtance of the Moon from the ſtar; the ſum, abating 20 from the index, is the tangent of an arch, which call A. Then, if the zenith diſtance of the Moon is greater than that of the ſtar, take the Sum of the arch A, juſt found, and half the diſtance of the Moon from the ſtar; but, if the zenith diſtance of the Moon be leſs than that of the ſtar, take the difference of the ſaid arch A and half the diſtance of the Moon from the ſtar; and the ſum, or difference call B. To the tangent of B, thus found, add the coſine of the Moon's zenith diſtance, and the logarithm of the Moon's horizontal parallax, expreſſed in minutes and decimals; the ſum, abating 20 from the index, is the logarithm of the effect of parallax, tending always to augment the apparent diſtance of the Moon from the ſtar; except the zenith diſtance of the Moon be leſs than that of the ſtar, and, at the ſame time, the arch A be greater than half the diſtance of the Moon from the ſtar, in which caſe the effect of parallax diminiſhes the apparent diſtance of the Moon from the ſtar.

DEMONSTRATION.

In the ſpherical triangle Z L S, ſee Fig. 2, 3, 4, and 5th, Z repreſents the zenith, L the Moon, and S the ſtar; the effect of parallax depreſſing the Moon from L to r, r is the apparent place of the Moon, and r S the apparent diſtance of the Moon from the ſtar; let fall the perpendicular Lt upon r S, produced if neceſſary, and rt will be the difference of L S and r S, or the effect of parallax. Draw the arch Z P perpendicular to to r S, and let M be the middle of r S. The Moon's parallax in altitude, being to her horizontal parallax, as the ſine of her apparent zenith diſtance, to the radius, L r = Moon's horizontal parallax × ſine of Zr; and rt the effect of parallax upon the apparent diſtance of the Moon from the ſtar will be = Lr × coſ. ZrS = horizontal parallax × ſin. Zr × coſ. ZrS (or, becauſe tan. rP : coſ. ZrP :: tan Zr : rad :: ſin Zr : cos Zr; and therefore ſin. Zr × cos ZrS = coſ. Zr × tan rP) = horizontal parallax × coſ. Zr × tan. rP agreeably to the rule. For it is evident by ſpherics that the arch A, found by the rule, is the ſame with M P the diſtance of the perpendicular from the middle of the arch rS: and it is evident, by the inſpection of the figures, that the arch B or r P is equal to the ſum of rM and M P, if the zenith diſtance of the Moon be greater than that of the ſtar, as in Fig. 2d and 4th; but is the difference of r M and M P, if the zenith diſtance of the Moon be leſs than that of the ſtar, as in Fig. 3d and 5th. Laſtly, it may appear from the

conſideration

consideration of the figures, that, as the effect of parallax depresses the Moon directly towards the horizon, so it will always encrease her apparent distance from a star, except in the case represented by Fig. 5th; that is to say, unless the zenith distance of the Moon be less than that of the star, and, at the same time, the arch MP be greater than rM or half the distance of the Moon from the star. Q. E. D.

Remarks on the use of the two foregoing rules.

It has been remarked, after the rule for refraction above, that if the altitudes of the Moon or star are under 10 degrees, the zenith distances must be first lessened by 3 times the refractions corresponding to their respective altitudes before the effect of refraction be computed.

But in order to compute the effect of parallax from the second rule, the observed distance of the Moon from the star must be first corrected by adding the effect of refraction to it, found by rule the first; as must the observed altitudes of the Moon and star be also corrected by taking from them their respective refraction in altitude, and the corrected arches thus found must be made use of in computing the parallax. Only, if the altitudes of the Moon and star are both 10 degrees or more, part of the calculation of rule the second may be saved, and arch the second, found by rule the first, taken for arch A in the second rule without any sensible error. In this case, it will be most convenient to observe the following order of computation instead of that before

prefcribed to be ufed when the altitudes are under 10 degrees.

1ft. Making ufe of the apparent altitudes of the Moon and ftar uncorrected, compute arches the firft and fecond by the directions contained in the rule of refraction.

2dly. Taking arch the fecond for arch A in the rule of parallax, compute the effect of parallax according to rule the fecond.

3dly. With arches the firft and fecond compute the effect of refraction by rule the firft.

4thly, and laftly. Applying the two corrections of parallax and refraction duly, according to the rules, to the obferved diftance of the Moon from the ftar, you will have the true and correct diftance of the Moon from the ftar, cleared both of refraction and parallax.

A RULE

For computing a fecond, but fmaller correction than the firft, neceffary to be applied to the obfervations of the diftance of the Moon from a ftar on account of parallax.

Call the principal effect of parallax, found by the preceding rule, the parallax in diftance; and find the parallax anfwering to the Moon's altitude. Then to the conftant logarithm 0.941 add the logarithm of the fum of the parallax in altitude and the parallax in diftance, the logarithm of the difference of the fame parallaxes, and the cotangent of the obferved diftance of the Moon from the ftar (corrected for refraction, and the principal effect of parallax), the fum, abating 13 from the index, is the logarithm

logarithm of the number of ſeconds required, being the ſecond correction of parallax; and is always to be added to the diſtance of the Moon from the ſtar, firſt corrected for refraction, and the principal effect of parallax found above, in order to obtain the true diſtance; unleſs the diſtance exceeds 90 degrees, in which caſe it is to be ſubſtracted.

DEMONSTRATION.

Let L Fig. 6. repreſent the Moon's true place in the ſphere, and *r* her apparent place as depreſſed by parallax, S the place of the ſtar, and L *t* a perpendicular let fall from the true place of the Moon L upon the great circle *r* S joining the ſtar S and the apparent place of the Moon *r*; (all as in the four figures belonging to the preceding rule). Let L *a* be the arch of a parallel circle deſcribed from the ſtar S as a pole through the true place of the Moon L. S *a* terminated by the parallel circle L *a*, and not S *t* terminated by the perpendicular L *t*, as was ſuppoſed in the former demonſtration, is equal to S L or the true diſtance of the Moon from the ſtar, which was therefore computed too ſmall from the former rule by the little ſpace *a t*. Let L T and *a* T be the equal tangents of the equal arches L S and *a* S in L and *a*, meeting in the radius C S, drawn from the centre of the ſphere C and produced, in T. The ſpace L *a t*, on account of its ſmallneſs, may be looked upon as lying all in one plane namely L *a* T, and L *a* as the ſmall arch of a circle deſcribed from the point T as a centre with the line L T as a radius, thro'

through L and a, Lt as the fine, and at as the verfe-fine of the arch La; and confequently at equal to the fquare of Lt divided by 2 L T. But, the triangle Lrt being right-angled in t, the fquare of Lt is equal to the difference of the fquares of Lr and rt, and confequently to the product of their fum and difference; that is to fay, $at = \frac{\overline{Lr+rt}\times\overline{Lr-rt}}{2\,LT}$ or (becaufe the tangent T L is equal to the fquare of the radius C S divided by the cotangent of L S) $= \overline{Lr+rt}\times\overline{Lr-rt}\,\frac{\times\ \text{cotangent of } LS}{2\times \text{fquare of } CS}$. Now fuppofe the fpaces Lr, rt to be expreffed in minutes, which will be moft convenient in practice, then the radius of the fphere C S muft be taken equal to $3437\frac{3}{4}$, for fo many minutes are contained in an arch of a circle equal to its radius: and at will be $= \frac{\overline{Lr+rt}+\overline{Lr-rt}\times \text{cotan. of } LS}{2\times 3437\frac{3}{4}\times 3437\frac{3}{4}}$. But, the contangents of fimilar arches of circles of different radii being directly as the radii, therefore the cotangent of L S to the radius C S or $3437\frac{3}{4}$, is to the cotangent of the fame arch to 10000000000, which is the radius to which the logarithmic tables are adapted, its logarithm being 10; as $3437\frac{3}{4}$ to 10000000000. Therefore the cotangent of L S = tabular cotangent $\frac{\text{of } LS\times 3437\frac{3}{4}}{10000000000}$, which, being fubftituted in the value of at above, gives at, expreffed in minutes $= \frac{\overline{Lr+rt}+\overline{Lr-rt}\times \text{tabular cotangent } LS}{68755000000000}$; or, multiplying by 60, the value of at will come out in fe-

 conds

conds $= \frac{\overline{Lr + rt} \times \overline{Lr - rt} \times \text{tabular cotangent of } LS}{114600000000}$.
The logarithm of the denominator 114600000000 is 12,059, instead of subftracting which, when the operation is performed by logarithms, add 0,941 (its compliment to 13) to the value of the numerator found in logarithms, and fubftract 13 from the index: the remainder will be the value of *at* in feconds. Q. E. D.

A concife rule to find the diftance of the Moon from a zodiacal ftar, very nearly; the difference of the longitudes of the Moon and ftar, and the latitudes of both being given.

To the cofine of the difference of the longitudes add the cofine of the difference of the latitudes, if both of the fame denomination, or fum; if of contrary denominations, the fum of the two logarithms, abating 10 from the index, is the cofine of the approximate diftance. This gives the true diftance of the Moon from the Sun, being then nothing more than the common rule for finding the hypothenufe of a right-angled fpherical triangle from the two fides given. But in the cafe of a zodiacal ftar apply the following correction to the approximate diftance thus found.

To the conftant logarithm 5.3144 add the fine of the Moon's latitude, the fine of the ftar's latitude, the verfe-fine of the difference of longitude, and the cofecant of the approximate diftance; the fum of thefe 5 logarithms, abating 40 from the index, is the logarithm of a number of feconds, which fubftracted

ſtracted from the approximate diſtance, found before, if the latitudes of the moon and ſtar are of the ſame denomination, or added thereto, if they are of different denominations, gives the true diſtance of the Moon from the ſtar.

N. B. This rule, though only an approximation, is ſo very exact, that even, if the latitude of the Moon was 5°, and that of the ſtar 15°, the error would be only 10″; and if the latitude of the Moon be 5°, and that of the ſtar 10°, the error is only $4'' \frac{1}{2}$; and if the latitudes be leſs, will be leſs in proportion as the ſquares of the ſines of the latitudes decreaſe.

DEMONSTRATION.

Let P [Fig. 7.] repreſent one of the poles of the eclip- and Q, R the places of the Moon and ſtar. From R let the arch of a great circle R D be drawn perpendicular to P Q. By ſpherics, the tangent of P D = tangent of P R × coſine of the angle R P D. And, by trigonometry, coſine of Q D or $(QP - PD)$ $= \text{coſ.}\ QP \times \text{coſ.}\ PD + \text{ſin.}\ QP \times \text{ſin.}\ PD = \text{coſ.}\ QP \times \text{coſ.}\ PD + \text{ſin.}\ QP \times \text{coſ.}\ PD \times \text{tan.}\ PD = \text{coſ.}\ PD \times \overline{\text{coſ.}\ QP + \text{ſin.}\ QP \times \text{tan.}\ PD} = \text{coſ.}\ PD \times \overline{\text{coſ.}\ QP + \text{ſin.}\ QP \times \text{tan.}\ PR \times \text{coſ.}\ P}$ $\therefore \text{coſ.}\ QD : \text{coſ.}\ PD :: \text{coſ.}\ QP + \text{ſin.}\ QP \times \text{tan.}\ PR \times \text{coſ.}\ P : 1$. But, by ſpherics, $\text{coſ.}\ QD : \text{coſ.}\ PD :: \text{coſ.}\ RQ : \text{coſ.}\ PR \therefore \text{coſ.}\ RQ : \text{coſ.}\ PR :: \text{coſ.}\ QP + \text{ſin.}\ QP \times \text{tan.}\ PR \times \text{coſ.}\ P : 1$. Whence $\text{coſ.}\ RQ = \text{coſ.}\ PR \times \text{coſ.}\ QP + \text{ſin.}\ QP \times \text{ſin.}\ PR \times \text{coſ.}\ P$: Now, by trigonometry, $\text{coſ.}\ (QP - PR)$

 = coſ.

$=$ cos. QP $\times$ cos. PR $+$ sin. QP $\times$ sin. PR; whence sin. QP $\times$ sin. PR $=$ cos. (QP $-$ PR) $-$ cos. QP $\times$ cos. PR; which being substituted above, gives cos. RQ $=$ cos. (QP $-$ PR) $\times$ cos. P $-$ cos. PR $\times$ cos. QP $\times$ cos. P $+$ cos. PR $\times$ cos. QP $=$ cos. (QP $-$ PR) $\times$ cos. P $+$ verse-sine P $\times$ cos. PR $\times$ cos. QP. Now put cos. (QP $-$ PR) $\times$ cos. P $=$ cos. G, or the approximate distance, then cos. RQ $-$ cos. G, or (because the difference of RQ and G is but small) $\overline{G - RQ} \times$ sin. $\left(\frac{G+RQ}{2}\right)$ $=$ verse-sine P $\times$ cos. PR $\times$ cos. PQ nearly. Whence RQ $=$ G $-$ $\frac{\text{verse-sine P} \times \text{cos. PR} \times \text{cos. QP}}{\text{sin.} \frac{G + RQ}{2}}$ nearly $=$ G $-$ $\frac{\text{v. s. P} \times \text{cos. PR} \times \text{cos. QP}}{\text{sin. G}}$ nearly. Q. E. D.

Note, the error of this formula arises from taking G $=\frac{G + RQ}{2}$ by which means it will always give RQ too great, nearly by the following quantity, $\frac{1}{2}$ S q cos. PR $\times$ S q cos. QP $\times$ cot. G $\times$ S q tan. $\frac{1}{2}$ G. This comes to a maximum when G is 60°, and is then $= \frac{1}{6} \sqrt{\frac{1}{3}} \times$ S q cos. RP $\times$ S q cos. PQ. If the latitudes of the Moon and star are both 5° it is $=$ 1″. If the Moon's latitude be 5°, and that of the star 10°, it is $=$ 4″ $\frac{1}{2}$; and if the latitude of the star be 15° it is $=$ 10″

XLIX.

XLIX. *Extract of a Letter from Mr.* John Winthrop, *Professor of Mathematicks in* Cambridge, New England, *to* James Short, *A. M. F. R. S.*

S I R, Dated June 6, 1764.

Read Nov. 15, 1764.

I AM greatly obliged to you for your candid and judicious remarks on my observation of Venus on the Sun, which I received from my much-esteemed friend Dr. Franklin. I wrote to the Dr. pretty largely on the subject, which I desired him to communicate to you: but when I had the pleasure of a visit from him last summer, he could not recollect whether he had done it or not. I therefore beg leave now to trouble you with the substance of it. Your remarks turned on two points, the longitude of the place of observation, and the equation of time when found by equal altitudes. As to the first, I was so diffident of the observation on the Moon, that I chose to keep to the longitude of St. John's, as set down by Sir Jonas Moore, who makes it 52° 50′ West of Greenwich. Though I did not think it needful to mention this doubt in the pamphlet, which was published soon after I got home, to gratify the curiosity of my countrymen, yet I expressed it fully in a written account of the observation, drawn up in a different form, and sent to the late Dr. Bradley, but which I believe never reached his hands.

As-

As to the equation of the time of noon, depending on the change of the Sun's declination, I did not make it in that pamphlet. I had all along intended to calculate it when I should settle the result of the observation, though I knew it must be very small, as the Sun did not alter his declination, then, above 1′ in 4 hours. But when I came to observe the contacts, and found that I could not be sure of them within 3″ or 4″, whereas Dr. Halley's papers led me to expect that I might observe them to a single second, I thought it hardly worth while to calculate this equation, as the precise moments of the contacts could not be fixed by it.—But I have since done it, and find it to be 4″ to be substracted from the middle time. An account of the observation, thus corrected, I sent last summer to Professor Bliss at Greenwich, concluding, from his station, that the observations from different parts of the world would be collected and compared by him. But having since understood that that affair is in your hands, though I have not yet had the pleasure of seeing your paper upon it, and having had no return from Mr. Bliss, I ask leave to transmit a copy of it to you; and if you will be pleased to give it a place in your Transactions with a remark at the end (if you think it proper) showing what the Sun's parallax comes out from the observation as it now stands (which I suppose will not differ much from the other determinations) I shall take it as a favour. This will do honour to the government who employed me. Perhaps, too, posterity may be glad to see, and may make use of, the only observation of this rare phænomenon that was made in America. I am, &c.

John Winthrop.

L. *Obser-*

L. *Obſervation of the Tranſit of* Venus, June 6, 1761, *at* St. John's, Newfoundland: *By* John Winthrop, *Profeſſor of Mathematicks and Philoſophy at* Cambridge, New England.

Read Nov. 15, 1764.

THE tranſit of Venus over the Sun, being a very curious and important phœnomenon, engaged the attention of America as well as Europe. His excellency Francis Bernard, Eſq; governor of the Maſſachuſetts-Bay, a gentleman who ſeizes every opportunity of advancing the ſciences, was deſirous to have an obſervation of it in this quarter of the world; and as Newfoundland was the only Britiſh plantation where one could be had, propoſed to the General Aſſembly at Boſton to make proviſion for that purpoſe, which they readily agreed to do. In conſequence whereof, I imbarked on board a veſſel in the ſervice of the government, taking with me for aſſiſtants two young gentlemen my pupils; and ſuch aſtronomical inſtruments out of the college apparatus as were neceſſary. Theſe were, an excellent clock, Hadley's octant with nonius diviſions; a refracting teleſcope with wires at half right angles, for taking differences of right aſcenſion and declination; and a nice reflecting teleſcope, adjuſted by croſs levels, and having vertical and horizontal wires, for taking correſpondent altitudes; or differences of altitudes and azimuths.

We

We arrived at St. John's Newfoundland the 22d of May, where we met with a very kind reception, and all the aſſiſtance we could deſire. As this town is bounded with high mountains toward the ſun-riſing, ſo that no houſe in it would anſwer our end, we encamped on an eminence at ſome diſtance, from whence we could ſee the Sun preſently after his riſing.

Hither we conveyed our inſtruments, and ſecured the clock to a pillar ſet in the ground under a tent. Near this tent, and within call of the clock, we fixed two other pillars firmly in the ground; one, to mount the refracting teleſcope on; the other, which was above 8 feet high, for a ſtyle or gnomon, having at top a plate of lead with a little hole for tranſmitting the Sun's rays; and we laid an horizontal platform to receive thoſe rays. The platform we kept covered, to defend it from the Sun and weather: and examined its poſition every time we made uſe of it, by a very long level. On this we carefully drew a meridian line, by correſpondent altitudes of the Sun, taken both by the reflector and by the Sun's image on the platform. Theſe operations we repeated every fair day, and ſeveral times in a day. It would be tedious, as well as needleſs, to give a detail of them: 'tis ſufficient that we adjuſted the clock with as much exactneſs as we could have done at home.

Thus prepared, we waited for the critical hour, which proved favourable to our wiſhes. The morning was ſerene and calm. The Sun roſe behind a cloud that lay along the horizon, but ſoon got above it; and at $4^h\ 18'$ we had the pleaſure of ſeeing *Venus on the Sun*; though dimly indeed at firſt. But

the planet presently became distinct, and her limb well defined. Upon this, I applyed myself to observe the passage of the Sun's and Venus's preceding limbs, by the vertical, and of their lower limbs by the horizontal, wires in the reflector, and made the following observations; one of my assistants counting the clock, and the other writing down the observations as I made them; which, having made the proper correction of the time for the change of the Sun's declination, stand as follows.

True time. h	′	″		diff. long. ☉ & ♀ ′	″	♀ Lat. South. ′	″
At 4	21	20	Sun at the vertical ——			10	47
		31	Venus at the same ——	9	2		
	23	6	Venus at the horizontal –	9	4	10	50
	24	23	Sun at the same. ——				
	27	29	Venus at the horizontal –	9	25	10	52
	28	47	Sun at the same ——				
	35	15	Sun at the vertical ——			10	55
		21	Venus at the same ——	9	56		
	37	49	Venus at the horizontal –	10	8	11	00
	39	9	Sun at the same — ——				

As Venus began now to draw near the Sun's limb, I prepared to observe her egress. The interior contact did not appear so perfectly instantaneous, as Dr. Halley's papers led me to expect. I was not certain of it till $4^h\ 47'\ 21''$, though I doubted of it at $17''$. The exterior contact I judged to be at $5^h\ 5'\ 49''$, doubtful also 3 or $4''$; and so the passage of Venus's diameter, $18'\ 28''$.

The above observations gave me several altitudes and azimuths of Venus, from whence I deduced her

 right

right aſcenſions and declinations; and from them, her longitudes and latitudes. The reſult of the whole, or the planet's difference in longitude from the Sun's centre and her latitude, is ſet down above, againſt each obſervation of Venus.

From hence I concluded that at the central emerſion, which I put at $4^h\ 56'\ 38''$, the difference of longitude was $11'\ 19$, and the latitude $11'\ 6''$. Alſo, that the conjunction in longitude happened at $2^h\ 4'\ 36''$, the planet's latitude then being $9'\ 28''$.

In theſe calculations, I ſuppoſed the ſemidiameter of the Sun to be $15'\ 50''$, and of Venus $29''$.

By ſeveral obſervations, I found the latitude of the place $47°\ 32'$ N; which falls within the latitudes laid down in ſeveral books and maps, which make it from $47°\ 25'$ to $48°\ 0'$. I could make no uſe of Jupiter's ſatellites in finding the longitude, as they were not riſen high enough to be obſerved above an hour before day-light came on. There were but two of their eclipſes that could have been viſible there while I was on the iſland; and though I watched for both of them, I was diſappointed of both by unfavourable weather. Neither was I fortunate enough to get ſo much as one occultation of a fixed ſtar by the Moon, though I ſpared no pains for it. The only obſervation I could get for this purpoſe was of the right aſcenſion of the Moon, which I endeavoured to find, by comparing with that of a fixed ſtar. But whether any miſtake was committed in counting the clock, or in writing down the obſervations, or whether the poſition of the teleſcope was diſturbed by any accident in the interval between the Moon's and ſtar's paſſing, I am not able now to ſay. However, as I am ſenſible that

that obſervation is not to be depended on, I think it needleſs to inſert it here. The longitude of St. John's is variouſly ſet down by different authors, though none I have met with mention the obſervations by which it was determined. According to Sir Jonas Moore, it is $52^h\ 50'$ Weſt from Greenwich; and as his authority may be as good as any, I keep to this longitude till it can be aſcertained by farther obſervations. I have taken meaſures to procure ſuch; and if they ſucceed, ſhall be ready to communicate the reſult.

I viewed the Sun with great attention in the reflector both on the 5th and 6th of June, in hopes to find a ſatellite of Venus; but in vain. There were ſeveral ſpots then on the Sun; but none that I ſaw could be a ſatellite.

The variation of the needle there I found $19°$ W.

J. Winthrop.

REMARK.

Mr. Short has computed the parallaxes at the egreſs for this obſervation at St. John's, and by comparing this obſervation with that at the Cape of Good Hope (on the above longitude and latitude of St. John's as ſet down by Mr. Winthrop) he finds the parallax of the Sun, reſulting therefrom, $= 8'',25$.

LI. *An Account of the Effects of Lightning on three Ships in the* East-Indies*: By Mr.* Robert Veicht. *Communicated by* William Lewis, *M. D. and F. R. S.*

Read Nov. 22, 1764.

AUGUST the 1st 1750. Lat. 1° 56′ N. Malacca bearing about N. E. All this day there was a fresh breeze, S. to S. S. E. the weather being hot and sultry. The evening was fair and clear; and when day-light was gone, there was not a cloud in the sky; and the water was so clear of vapour or mist, that the stars could be seen to rise out of the horizon.

This serenity continued till about 2 A. M. when a black cloud appeared above the horizon in the W. and W. N. W. and continued to rise very fast; and the flashes of lightening, which proceeded from it, succeeded each other very fast. In $\frac{1}{4}$ of an hour, it covered almost half the hemisphere, and as it approached, the wind from the S. E. began to fail, and died quite away at last.

By the time the clouds had covered half the hemisphere, the wind proceeded from it in great violence, and the flashes of lightening were very frequent, and we judged of their nearness to the ship by the interval betwixt the flash and report, according as this interval was longer or shorter. The whole heavens were now covered with this cloud, and the flashes of lightening happened at times on different sides of the ship, which had all the sails furled

furled before it came upon her. It muſt be remarked, that the wind, which reached the ſhip before the thunder, brought with it a violent and heavy rain, which ſufficiently ſoaked the ſhip and every thing about her. The ſhip was all this time, which was in about half an hour after its firſt appearance above the weſtern horizon, in the midſt of repeated flaſhes of lightening, which were juſt upon the ſhip by her trembling and ſhaking on every exploſion, and the flaſh and clap coming in the ſame inſtant, and the officers and people were apprehenſive of damage to the maſt.

2 ½ A. M. At this time a clap burſt, as was judged by the report, about mid-way betwixt the head of the maſt and the body of the ſhip, or it might be higher, and in deſcending might cauſe that appearance, and juſt over it. This made the ſhip tremble and ſhake as if ſhe was going to burſt into pieces, and great pieces and ſplinters of the maſt were fallen upon different places of the ſhip; but it was ſo very dark, we could not ſee from which of the maſts they were forced.

Immediately after this firſt came a ſecond, which burſt juſt above, and on the quarter deck of the ſhip, which by the report was as great, and being cloſe upon the deck was more terriſying than the former.

Here I muſt take notice, that the wind brought very heavy rain before the thunder came near the ſhip; and in proportion as the thunder approached the ſhip, the wind, which came in violent guſts at firſt, decreaſed gradually, and the rain was leſs heavy; and when the thunder ſurrounded the ſhip, and broke upon her, it was almoſt calm. And we could

could not only judge of the nearneſs of the thunder by the tremor and ſhaking of the ſhip, and the report inſtantaneouſly following the flaſh; but we could alſo hear ſeveral of the flaſhes fall into the water cloſe upon the ſhip.

We reckoned, that the firſt clap, which burſt at the main-maſt, was what damaged the maſt; the ſecond having burſt betwixt the main-maſt and mizen-maſt.

At this time we came to an anchor, and continuēd till day-light, that we might examine into the damage we had received; for, as pieces of the maſt were carried to all places of the ſhip, we imagined, that all the maſts had been hurt.

At day-light we found, that the fore-maſt and mizen-maſt had eſcaped, and the main-maſt had ſuffered as follows:

All the main-top-gallant-maſt (which is the uppermoſt piece of the maſt) from the rigging at the top of it, to the cap at the head of the main-top-maſt, was entirely carried away, part falling over-board, and part into the ſhip in different places. The main-top-maſt had great pieces carried from it, from the hunes down to the cap, at the head of the main-maſt, ſo that it could but juſt ſtand, being hardly ſtrong enough to bear its own weight, and that of its rigging. The main-maſt being compoſed of three pieces, towards the top of it, thoſe of the ſides, being of oak, called the cheeks, were not hurt; but the middlemoſt part, being of fir, was ſhivered in ſeveral places, and pieces were carried out out of it 6 or 7 inches in diameter, and from 10 to 12 feet long, and this in a circular deſcending manner from the parrel of the

the main-yard down to the upper deck of the ſhip, the pieces being taken out crooked, or circular, or ſtrait, according as the grain of the wood ran. It muſt be remarked, that theſe claps were not one ſingle exploſion, but ſucceſſive exploſions, about the dimenſions, as near as we could gueſs, of ſmall ſhells, and continued ſome time cracking after each other; and as the lightening is obſerved to run not in ſtrait line, but zig zag, ſo theſe different exploſions might be differently placed in the air; that when they came to take fire and burſt, they might take the pieces out of the different ſides of the maſt as above related.

In great ſhips the maſts are compoſed of three parts, erected upon one another, the lowermoſt part is called by its proper name, the middlemoſt part is called the top-maſt, and the uppermoſt part the top-gallant-maſt. The maſt, which was here damaged, was the main-maſt, or principal maſt of the ſhip, and which ſtands near the middle; and ſometimes the name of main-maſt is applied to all the three pieces as they ſtand erected, and ſometimes to the lower piece, or part of the maſt only: and when they are diſtinguiſhed ſeverally, they are called the main-maſt, main-top-maſt, and main-top-gallant-maſt.

Each of theſe parts of the maſt are divided as to length, and have their proper names accordingly; and generally into three parts in common converſation, *viz.* the head part, which reaches from the upper extremity to the place, where the rigging is fixt; the middle part, which reaches from a little below the rigging, to that place, where the lowermoſt part be-

gins, and this is often called the hoiſt, or hoiſting part; and the lowermoſt part, called the heel, reaches to the lower extremity. There is the ſame diviſion of all the three parts; of which the maſt is compoſed; but of the lowermoſt part of the maſt, the heel part of it is hid below the upper deck in the body of the ſhip.

At the top of the main-maſt, on the extremity, is fixed a piece of wood, which has a hole in it; and at the lower part of the head of the main-maſt are alſo fixed ſome pieces of wood croſs each other, on which lies a ſcaffolding called the top: through theſe the top-maſt is thruſt upwards; and when erected at its greateſt height, the lower part of the top-maſt, called the heel, and the upper part of the main-maſt lie cloſe to and againſt each other, and betwixt the top, or ſcaffold, and the cap: and ſo the top-maſt and top-gallant-maſt together: but the head part of the top-gallant-maſt, as there is no other above it, is tapered away to a point, whereon is fixed an iron ſpindle and vane.

As a ſhip does not at all times carry her ſail of the ſame height, but higher or lower in proportion to the ſtrength of the wind or other circumſtances, it is upon the middlemoſt of the three parts abovementioned, into which the maſts are divided as to length, that the ſails are made to ſlip up and down, and are attached to the maſt by ſeveral pieces of wood fixed by a rope round it to the yard, that extends the ſail, and this ſlips up and down along with the yard, and is called the parrel.

And as it is neceſſary to cover theſe maſts with ſome matter, that may preſerve them from the

weather

weather and ſun, they are therefore covered with different kinds of matter, according to the uſes of the different parts of the maſt.

The head and heel parts of the maſts are always covered with tar mixed into a conſiſtence with lamp black, and this being frequently repeated, it forms at laſt a covering of the thickneſs of a crown, and ſometimes a quarter of an inch; through which as the Sun cannot pierce, it is commonly without flaw or rent; but the middlemoſt part, upon which the ſail ſlips up and down, is always rubbed with tallow, or greaſe, or hogs lard, to make it more ſlippery; and this being frequently ſcraped off, and anointed afreſh, and in hot weather, or the ſummer time, it becomes extremely thin, being melted off, and frequently and in many places rubbed off by the ſlipping up and down of the ſail and parrel, which expoſes it ſo much, that the Sun ſometimes rends it from end to end almoſt, and ſo deep as to reach the centre of the maſt, and an inch wide. But as the yard, which extends the lowermoſt ſail upon any of the maſts, is commonly carried at the ſame height, the middlemoſt part of this maſt is uſually covered with roſin mixed with tallow or oil, and ſometimes turpentine mixed with the ſame; and this being ſoft, cracks and melts with the Sun, and ſo leaves ſome places of the maſt bare, and this will rend and ſplit alſo like the top-maſt and top-gallant-maſt, that are covered with tallow or greaſe, but not quite ſo large rents.

We are now to obſerve, that no part of the top-gallant maſt or top-maſt, that was covered with the lamp black were touched with the thunder, the

 greaſy

greaſy part only being carried away. The head of the top-gallant-maſt, from the rigging upwards to the ſpindle, was entire, as was alſo its heel, for the lightening did not touch the heel, but miſſed the whole both of top-gallant-maſt and top-maſt, that lay betwixt the cap and upper end of the greaſy part of the maſt. Of the top-maſt great pieces were carried out, of many feet in length, and 9 or 10 inches in thickneſs, and this on different ſides of the maſt, for the whole length of the greaſy part. From the top of the main-maſt to the upper end of that, which is covered with turpentine, there was no damage; but, from thence downwards, the cheeks were ſtarted off from the middle part, and pieces taken out winding aſlant down the maſt, and out of the fir part many feet in length, and 6 and 7 inches deep, and near the upper deck a piece as large as the body of a man, and 11 or 12 feet in length.

I imagine, that the vapour having inſinuated itſelf into the rents and cracks of the maſt, takes fire, and expanding itſelf every way, is the occaſion of pieces of the maſt being carried to the moſt diſtant parts of the ſhip; and, as a proof of this, the oak cheeks above-mentioned, though extremely well fixed to the middle part of the maſt, which was of fir, by ſpikes and bolts, which were clinched, they having ſhrunk, and thereby having left the jointure a little open, had made way for the vapour to inſinuate itſelf, which might be the occaſion, that the cheeks were ſtarted off at the lower part, and the rope, wherewith they were woolded together to the middle part in four different places, with 12 turns of 2 $\frac{3}{4}$ inch rope in circumference, were burſt all to pieces.

It muſt likewiſe be remarked, that the yards, which lay in a horizontal poſition, were not touched or hurt. Indeed they are always covered with lamp black and tar, as the heels and heads of the maſt are, and this frequently repeated; yet they had many and large rents in them; but whether the matter, which covered them, or their horizontal poſition, was the reaſon of their eſcaping the thunder, is left to the learned to determine. For in this ſituation you have nothing to do but to ſit with your arms acroſs, and compoſe yourſelf in expectation of your fate.

I muſt alſo take notice, that no part of the rigging was hurt; for although the middle part of the top-gallant-maſt, which was 18 feet long, and 9 inches diameter, was entirely burſt to pieces, and carried away; yet the rigging, which ſurrounded the upper part, was neither burnt, ſcorched, nor broke. Neither did it touch the caps on the maſt heads, nor the top, or round ſcaffolding on the maſt, and in this ſhip it was 18 feet broad; and theſe as well as the yards were covered with tar and lamp black, and made of three inch deal.

I muſt likewiſe take notice, that upon the upper deck of the ſhip are two convenient boxes built, divided into two and into three parts, wherein are placed a lamp, which burns in the night, and a compaſs, whereby the ſhip is ſteered. One of theſe was placed very near to the main-maſt in the middle of the ſhip, and the other cloſe to the mizzen-maſt, and both the lamps were burning at the time of the firſt exploſion; and immediately, upon orders given, all the lights in the ſhip were extinguiſhed before the burſting of the ſecond clap; the officers imagining the inflammable

flammable vapour might be attracted by the flame of the lamp and fired thereby.

At the time of the first, I believe, there might be more than sixty men upon deck, and some of them very near the mast at the very time of the clap. Some of these were stunned and beat down; and in their arms, where they thought themselves hurt, they had a numbness, which continued some time, but not any of them otherwise hurt. Luckily before the second, the men, who were upon the quarter deck, in number about twenty, had time to retire under the auning, which is a projection of the deck of the cabin to shelter from the sun or rain; so all escaped unhurt, though sufficiently frighted. And indeed the second flash was most terrible, as it was an explosion of a great number of balls, which went off after each other, cracking like shells, which continued for the space of half a minute; and from which there was no retiring, as the door of the cabin was shut; and I believe they might have set the ship on fire, but for the great rain, which had fallen immediately before this. After this time we were in no more danger this night, the thunder being all past the ship, less frequent, and not so loud, and removing by degrees to a greater distance: and by day-light, which is here a little after five, the sky was quite cleared; a fine day; and the wind returned to the S. E. quarter.

In these cases of thunder there is not any precaution taken farther, than stopping the upper part of the pumps, because they pierce all the decks even to the outside plank in the bottom of the ship. If at sea, the sails are for the most part taken in; and

in port the men are ordered under cover, and the hatches are laid over and covered. The ſcuttle to the powder-room is well covered with wet ſwabs, and the paſſage ſecured.

Before I end this account, I ſhall give you the relation of an accident from thunder at Batavia.

Anno 1746. A Dutch ſhip, lying in the road of Batavia, having taken leave of the governor, was ready to depart for Bengal. The afternoon was calm, and towards evening they had looſed their ſails, and lay ready to take up their anchor upon the coming off of the wind from the land, which is common every night. A black cloud was gathering over the hills, and the wind brought it towards the ſhip: by the time the cloud and the wind reached the ſhip, a clap of thunder burſt from it juſt over the ſhip, and ſet fire to the main-top-ſail, which being very dry, burnt with great fury; and this ſet fire to the rigging and maſt. They immediately attempted to cut away the maſt, but were hindered by the falling of the rigging, which was burnt, from the head of the maſt. By degrees the fire communicated to the other maſts, and obliged the people to deſert the ſhip; and afterwards it took hold of the body of the ſhip, and burning down to the powder, the upper part of the hull blew up, and the bottom part ſunk in the place, where ſhe was at anchor.

Anno 1741. Bencoolen road on the S. W. ſide of the Iſland of Sumatra, Lat. 4° 0′ South. There lay here two ſhips, one an European, the other a country trading ſhip, both belonging to the Eaſt

India

India Company. Here, as well as in the ſtreight of Malacca, you have periodical winds, which blow for ſix months of the year from the ſame quarter of the horizon, and the other ſix months from the oppoſite quarter; and it is obſervable, that theſe thunder-ſhowers and ſqualls of wind uſually come contrary to theſe ſtated winds, which are calmed during the thunder, but return to their conſtant quarter as ſoon as the thunder and rain are paſt. If I recollect aright, in the above year 1741, in June, the weather was very hot and ſultry, and the conſtant wind but very faint. The wind came after this from the land, and almoſt oppoſite to the uſual point a very faint air; and the thunder was frequent and cloſe to the ſhips, which lay near each other, but the fog and rain prevented their ſeeing each other; but they often trembled and ſhook by the exploſion of the thunder. One of theſe claps burſt upon the country ſhip, which by this time had her top-maſts ſtruck; that is, lowered down along the lower-maſts. This clap carried away and burſt to pieces all the part of the lower-maſt from where the yard is caried aloft to within ſix or ſeven feet of the upper deck. The maſt was woolded with ropes of 2 $\frac{1}{2}$ ſize in different places, which were burſt aſunder at every turn of it; and the maſt all ſhivered into ſmall ſplinters, and moſtly carried overboard. Here alſo the main-maſt was made of fir, and the part, which was ſplit and ſhivered to pieces, was the part uſually coated with turpentine mixed as before-ſaid with tallow or oil: and the main-top-maſt, which was made of a wood of the country called teak, and is of a texture like to oak, but ſtronger, was untouched, notwithſtanding it

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it lay parallel, and touched the maſt for the whole length of the part carried away. Here the vapour muſt be very low, being wholly below the top, which was unhurt, as was all the rigging and yards of this particular maſt. And from the upper deck of the ſhip to the top could not, in this ſhip, which was but ſmall, exceed 42 or 44 feet. And in this laſt caſe the exploſion muſt have been like gunpowder endeavouring to expand itſelf every way, becauſe the top and top-maſt, and rigging, which was above it, ſunk perpendicularly down on the body of the ſhip, as did the top-maſt alſo, which pierced the upper deck, and ſtood upright. In this ſhip there might be at the time of the exploſion ſeventy men upon deck, and not one of them hurt; which I imagine was owing to the vapour being diſtant above the deck more than the height of a man, as was apparent from a remnant of the maſt, which was not touched for ſix or ſeven feet immediately above the deck.

The relater was an eye-witneſs to both theſe accidents. In this laſt there remained a ſtinking vapour for ſome time; but in the firſt caſe there was not the leaſt ſmell of ſulphur, or any other thing.

Robert Veitch.

LII. *A Demonstration of the Second Rule in the Essay towards the Solution of a Problem in the Doctrine of Chances, published in the Philosophical Transactions,* Vol. LIII. *Communicated by the Rev. Mr.* Richard Price, *in a Letter to Mr.* John Canton, *M. A. F. R. S.*

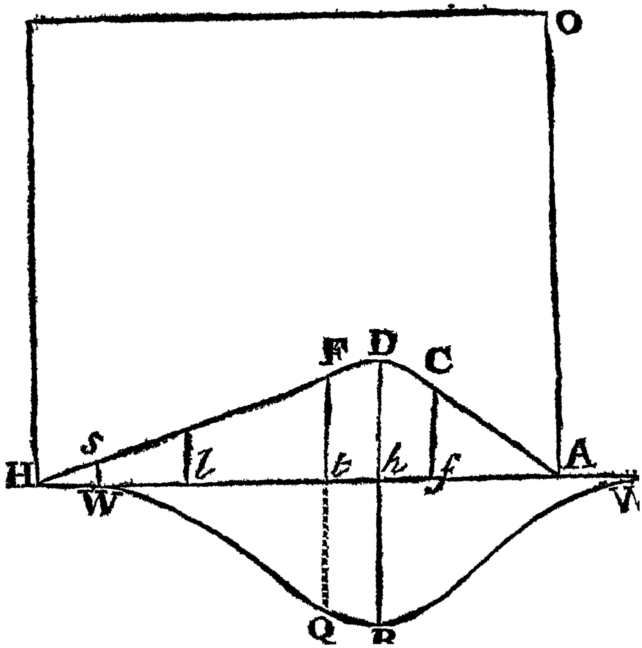
Dear Sir, Nov. 26, 1764.

Read Dec. 6, 1764.

I Send you the following *Supplement to the Essay on a Problem in the Doctrine of Chances*, hoping that you may not think it improper to be communicated to the Royal Society. I should not have troubled you again in this way had I not found that some additions to my former papers were necessary in order to explain some passages in them, and particularly what is hinted in the note at the end of the Appendix. "I have first given the deduction of Mr. Bayes's second rule chiefly in his own words; and then added, as briefly as possible, the demonstrations of several propositions, which seem to improve considerably the solution of the problem, and to throw light on the nature of the curve by the quadrature of which this solution is obtained." Perhaps, there is no reason for being very anxious about proceeding to further improvements. It would, however, be very agreeable to me to see a yet easier and nearer approximation to the value of the two series's in the first rule: but this I must leave abler persons to seek, chusing now entirely to drop this subject. The

The ſolution of the problem enquired after in the papers I have ſent you has, I think, been hitherto a *deſideratum* in philoſophy of ſome conſequence. To this we are now in a great meaſure helped by the abilities and ſkill of our late worthy friend; and thus are furniſhed with a neceſſary guide in determining the nature and proportions of unknown cauſes from their effects, and an effectual guard againſt one great danger to which philoſophers are ſubject; I mean, the danger of founding concluſions on an inſufficient induction, and of receiving juſt concluſions with more aſſurance than the number of experiments will warrant. I am, under a ſenſe of the value of your friendſhip, heartily yours,

Richard Price.



ART. 1. If the curve ADH be divided into two parts by the ordinate D*b* making A*b* to H*b* as *p* is to *q*; then taking $a = \frac{p}{n}$ and $b = \frac{q}{n}$ the ratio of the Area AD*b* to HO will be $\frac{a^{p+1} \times b^{q}}{p+1} \times \overline{1 + \frac{q}{p+2} \times \frac{p}{q} + \frac{q \times \overline{q-1} \times p^2}{\overline{p+2} \times \overline{p+3} \times q^2} + \frac{q \times \overline{q-1} \times \overline{q-2} \times p^3}{\overline{p+2} \times \overline{p+3} \times \overline{p+4} \times q^3}}$ + &c. For the series $\frac{x^{p+1} r^{q}}{p+1} + \frac{q}{p+1} \times \frac{x^{p+2} r^{q-1}}{p+2}$ + &c. in Prop: 10. Art. 2. of the Essay, which expresses the ratio of AC*f* to HO, becomes this series when $x = a = \frac{p}{n}$, $b = r = \frac{q}{n}$; that is when

C *f*

C f has moved till it coincides with D b and A C f becomes A D b. In like manner, from Art. 3. in the Essay, it appears that the ratio of H D b to H O is $\frac{a^p b^{q+1}}{q+1} \times \overline{1 + \frac{p}{q+2} \times \frac{q}{p} + \frac{p}{q+2} \times \frac{p-1}{q+3} \times \frac{q^2}{p^2}}$ + &c.

From hence it follows that the ratio of the difference between A D b and H D b to H O is $\frac{a^p b^q}{n}$ multiplied by the difference between the series $\frac{p}{p+1} + \frac{q}{p+1} \times \frac{p^2}{pq+2q} + \frac{q \times \overline{q-1} \times p^3}{\overline{p+1} \times \overline{p+2} \times \overline{pq^2+3q^2}}$ + &c. and the series $\frac{q}{q+1} + \frac{p \times q^2}{\overline{q+1} \times \overline{pq+2p}} + \frac{p \times \overline{p-1} \times q^3}{\overline{q+1} \times \overline{q+2} \times \overline{p^2 q+3p^2}}$ + &c. the former series being to be substracted from the latter, if H D b is greater than A D b, and *vice versa*.

2. The ratio of any term in the former of the two foregoing series to that which next but one follows the correspondent term in the latter is $\frac{pq+p}{p \times q} \times \frac{pq+2p}{p \times q} \times \frac{p \times q}{qp+q} \times \frac{pq+3p}{pq-q} \times \frac{pq}{pq+2q} \times \frac{pq+4p}{pq-2q} \times \frac{pq-p}{pq+3q} \times \frac{pq+5p}{pq-3q} \times \frac{pq-2p}{pq+4q} \times \frac{pq+6p}{pq-4q}$ &c. taking twice as many terms and four over as there are units in the number which expresses the distance of the term in the former series from its first term; which

ratio if q be greater than p, it is evident muſt be greater than the ratio of equality. Wherefore, if from the ſecond ſeries you ſubſtract the two firſt terms which together are leſs than two, the remainder is leſs than the former ſeries; and of conſequence, the former ſeries ſubſtracted from the latter cannot leave a remainder ſo great as two. And therefore in this caſe, that is, when q is greater than p, by the preceding article, the ratio of H D b — A D b to H O cannot be ſo great as $\underline{2a^{p}b^{q}}$.

3. The curve A D H being as before divided into two parts A D b and H D b, let the ordinates C f and F t be placed on each ſide of D b and at the ſame diſtance from it, and let z be the ratio of bf or bt to A H. Then if y, x and r be reſpectively the ratios of C f, A f and H f to A H, by the nature of the curve $y = x^{p}r^{q}$. But becauſe the ratio of A b to A H is a, and that of bf to A H is z, the ratio of A b—bf (= A f) to A H is $a-z$. Wherefore $a-z=x$. And in like manner $b+z=r$. But $y=x^{p}r^{q}$, and y is the ratio of C f to A H. Wherefore the ratio of C f to A H is $\overline{a-z}|^{p}\times\overline{b+z}|^{q}$. And in like manner the ratio of F t to A H is $\overline{a+z}|^{p}\times\overline{b-z}|^{q}$. And conſequently C f is to F t as $\overline{a-z}|^{p}\times\overline{b+z}|^{q}$ is to $\overline{a+z}|^{p}\times\overline{b-z}|^{q}$.

4. If q is greater than p, $\overline{a+z}|^{p}\times\overline{b-z}|^{q}$ is greater than $\overline{a-z}|^{p}\times\overline{b+z}|^{q}$, and the ratio between them increaſes as z increaſes. For the hyperbolic logarithm of

of that ratio taken as ufual, and then inftead of p and q putting $n\,a$ and $n\,b$ becaufe $a = \frac{p}{n}$ and $b = \frac{q}{n}$ (Vid. Art. 1.) you will find to be $2\,n$ multiplied by the feries $\frac{b^2 - a^2}{3\,b^2\,a^2} \times z^3 + \frac{b^4 - a^4}{5\,b^4\,a^4} \times z^5 + \frac{b^6 - a^6}{7\,b^6\,a^6} \times z^7$ &c. which logarithm when q is greater than p, and therefore b greater than a has all its terms pofitive, and fo much the greater as z is greater; and therefore it is the logarithm of a ratio greater than that of equality, and which increafes as z increafes.

5. By Art. 3. F t is to C f as $\overline{a+z}|^p \times \overline{b-z}|^q$ is to $\overline{a-z}|^p \times \overline{b+z}|^q$. And by Art. 4. $\overline{a+z}|^p \times \overline{b-z}|^q$ is greater than $\overline{a-z}|^p \times \overline{b+z}|^q$, and the ratio between them increafes as z increafes, if q is greater than p. Wherefore, upon this fuppofition, alfo F t is greater than C f, and the ratio between them increafes as z or $b\,t$ and $b\,f$ increafes, and confequently this will be true alfo concerning the areas defcribed by them as their equal abfciffes $b\,t$ and $b\,f$ increafe. Wherefore, when q is greater than p, D $b\,t$ F is greater than D $b\,f$ C, and the ratio between them increafes as $b\,f = b\,t$ increafes.

6. Becaufe A b is to H b as p is to q, when q is greater than p, H b is greater than A b. In H b therefore taking $b\,l$ equal to A b, by the preceding Art. the part of the figure H D b which infifts upon $b\,l$ will be greater than A D b, and the ratio of that part of H D b to A D b will be greater than the ratio of D $b\,t$ F to D $b\,f$ C. Confequently, much more (q being greater than p) the whole figure H D b is greater

greater than A D *b*, and the ratio of H D *b* to A D *b* is greater than that of D *b t* F to D *b f* C.

7. When q is greater than p, $\overline{1-\frac{n^2z^2}{pq}}\Big|^{\frac{n}{2}}$ is greater than $\overline{1-\frac{nz}{p}}\Big|^{p}\times\overline{1+\frac{nz}{q}}\Big|^{q}$ and less than $\overline{1-\frac{nz}{q}}\Big|^{q}\times\overline{1+\frac{nz}{p}}\Big|^{p}$. For the fluxion of $1-\frac{n^2z^2}{pq}$ is $\frac{n^3z\dot{z}}{pq}\times\overline{1-\frac{n^2z^2}{pq}}\Big|^{\frac{n}{2}-1}$ and the fluxion of $\overline{1-\frac{nz}{p}}\Big|^{p}\times\overline{1+\frac{nz}{q}}\Big|^{q}$ (because $p+q=n$) is $-\frac{n^3z\dot{z}}{pq}\times\overline{1-\frac{nz}{p}}\Big|^{p-1}\times\overline{1+\frac{nz}{q}}\Big|^{q-1}$. Wherefore $\overline{1-\frac{n^2z^2}{pq}}\Big|^{\frac{n}{2}}$ is to $\overline{1-\frac{nz}{p}}\Big|^{p}\times\overline{1+\frac{nz}{q}}\Big|^{q}$ as the fluxion of the former multiplied by $\overline{1-\frac{n^2z^2}{pq}}$ to the fluxion of the latter multiplied by $(\overline{1-\frac{nz}{p}}\times\overline{1+\frac{nz}{q}}$ or) $1-\frac{nz}{p}+\frac{nz}{q}-\frac{nz}{pq}$. From which analogy, because q is greater than p, it is plain that $\overline{1-\frac{nz}{p}}\Big|^{p}\times\overline{1+\frac{nz}{q}}\Big|^{q}$ varies at a greater rate in respect of its own magnitude than $\overline{1-\frac{n^2z^2}{pq}}\Big|^{\frac{n}{2}}$ does. And, because their fluxions as found out before have a negative sign before them, they both decrease as z increases;

creases; consequently, if they are ever equal, as z increases the latter must be the largest. But when $z = 0$ they are each equal to 1. In like manner the other part of this article appears. And hence, since $a = \frac{p}{n}$ and $b = \frac{q}{n}$, it is manifest that $a^p b^q \times \overline{1 - \frac{n^2 z^2}{pq}}\vert^{\frac{n}{2}}$ is greater than $\overline{a-z}\vert^{p} \times \overline{b+z}\vert^{q}$ and less than $\overline{a+z}\vert^{p} \times \overline{b-z}\vert^{q}$, when q is greater than p.

8. Suppose now further that the curve R Q W be described meeting the lines D h, F t, $h\,t$ produced in R, Q, W, in such manner that F t, which is to C f as $\overline{a+z}\vert^{p} \times \overline{b-z}\vert^{q}$ to $\overline{a-z}\vert^{p} \times \overline{b+z}\vert^{q}$ (Vid. Art. 3.) shall be to Q t as $\overline{a+z}\vert^{p} \times \overline{b-z}\vert^{q}$ to $a^p b^q \times \overline{1 - \frac{n^2 z^2}{pq}}\vert^{\frac{n}{2}}$ wherever the points t and f fall at equal distances from h. And it is manifest by the foregoing Art. that Q t must always be greater than C f, and less than F t. And of consequence the same must be true concerning the areas described by their motion while their equal abscisses increase. Wherefore R $h\,t$ Q is greater than D $h\,f$ C, and less than D $h\,t$ F.

9. Since F t is to Q t as $\overline{a+z}\vert^{p} \times \overline{b-z}\vert^{q}$ to $a^p b^q \times \overline{1 - \frac{n^2 z^2}{pq}}\vert^{\frac{n}{2}}$; and $\overline{a+z}\vert^{p} \times \overline{b-z}\vert^{q}$ (by

Art.

Art. 3.) expresses the ratio of Ft to A H; the ratio of Qt to A H must be $a^p b^q \times \overline{1 - \frac{n^2 z^2}{pq}}\Big|^{\frac{n}{2}}$, and as has been all along supposed z is the ratio of bt to A H. Wherefore, by squaring the curve RbtQ, it will appear that the ratio of R b t Q to H O is

$$a^p b^q \times \overline{z - \frac{n^3 z^3}{2.3pq} + \frac{n-2}{4} \times \frac{n^5 z^5}{2.5 p^2 q^2} - \frac{n-2}{4} \times \frac{n-4}{6} \times \frac{n^7 z^7}{2.7 p^3 q^3} + \&c.}$$

which $\left(\text{if } m^2 = \frac{n^3}{2pq}\right)$ is

$$a^p b^q \times \frac{\sqrt{2pq}}{n\sqrt{n}} \times \overline{mz - \frac{m^3 z^3}{3} + \frac{n-2}{2n} \times \frac{m^5 z^5}{5} - \frac{n-2}{2n} \times \frac{n-4}{3n} \times \frac{m^7 z^7}{7} + \frac{n-2}{2n} \times \frac{n-4}{3n} \times \frac{n-6}{4n} \times \frac{m^9 z^9}{9} - \&c.}$$

Which last series when $\frac{n^2 z^2}{pq} = 1$, and consequently the ordinate Qt vanishes, becomes $B - \frac{B^3}{3} + \frac{B^2-1}{2B^2} \times \frac{B^5}{5} - \frac{B^2-1}{2B^2} \times \frac{B^2-2}{3B^2} \times \frac{B^7}{7} + \&c.$ taking $B^2 = \frac{n}{2}$.

10. If $B^2 = \frac{n}{2}$ the ratio of the whole figure R Q W b to H O is $\frac{\sqrt{2pq}}{n\sqrt{n}} \times a^p b^q \times \overline{B - \frac{B^3}{3} + \frac{B^2-1}{2B^2} \times \frac{B^5}{5} - \&c.}$ Now, (by Prop. 10. Art. 4. of the Essay) the ratio of A C F H to H O is $\frac{1}{n+1}$

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$\times \frac{1}{E}$, E being the coefficient of that term of the binomial $\overline{a+b}^{n}$ expanded in which occurs $a^{p} b^{q}$: Wherefore, the ratio of R Q W b to A C F H is $\frac{n+1}{n} \times \frac{\sqrt{2pq}}{\sqrt{n}} \times \text{E}\, a^{p} b^{q} \times \overline{\text{B} - \frac{\text{B}^{3}}{3} + \frac{\text{B}^{2}-1}{2\text{B}^{2}} \times \frac{\text{B}^{5}}{5}\,\&c.}$ Put G now for the coefficient of the middle term of the ſame binomial, and if $p = q = \frac{n}{2}$, E $=$ G, $a = \frac{1}{2} = b$ the area R Q W b is equal to half A C F H; for then Q t, F t, C f are all equal, and conſequently the areas R Q W b, H D b and A D b. Wherefore, the ſeries $\text{B} - \frac{\text{B}^{3}}{3} + \&c.$ is equal to $\frac{\sqrt{2n}}{n+1} \times \frac{2^{n-1}}{\text{G}}$. But the ſeries $\text{B} - \frac{\text{B}^{3}}{3} + \&c.$ $\left(\text{becauſe B}^{2} = \frac{n}{2}\right)$ does not alter whatever p and q are, whilſt their ſum n remains the ſame. Wherefore, in all caſes, the ratio of R Q W b to A C F H is $\frac{\sqrt{pq}}{n} \times \frac{\text{E}\, a^{p} b^{q}}{\text{G}} \times 2^{n}$.

11. By Prop. 10. Art. 4. of the Eſſay, the ratio of A C F H to H O* is $\frac{1}{n+1} \times \frac{1}{\text{E}}$; and by Art. 9. the ratio of R b t Q to H O is $a^{p} b^{q} \times \frac{\sqrt{2pq}}{n\sqrt{n}} \times \overline{m z - \frac{m^{3} z^{3}}{3} + \frac{n-2}{2n} \times \frac{m^{5} z^{5}}{5}\,\&c.}$ Wherefore, the ratio of

* It is hoped that the imperfection of the figure all along referred to will be excuſed. The lines R b and D b ſhould appear equal; and it will be found preſently, that the curve line A C D F H ſhould have been drawn from F and C convex towards A H.

RbtQ to ACFH is $\frac{n+1}{n}\times\frac{\sqrt{2pq}}{\sqrt{n}}\times Ea^pb^q\times\overline{mz-\frac{m^3z^3}{3}+\frac{n-2}{2n}\times\frac{m^5z^5}{5}-\frac{n-2}{2n}\times\frac{n-4}{3n}\times\frac{m^7z^7}{7}+\&c.}$

Likewise, by Art. 10. the ratio of RQWb to ACFH is $\frac{\sqrt{pq}}{n}\times\frac{Ea^pb^q}{G}\times 2^n$. Wherefore the ratio of RbtQ to RQWb is $\frac{n+1}{\sqrt{n}}\times\frac{\sqrt{2}}{2^n}\times G\times\overline{mz-\frac{m^3z^3}{3}+\&c.}$

12. By Art. 2. 6. When q is greater than p, the ratio of HDb — ADb to HO is less than $\frac{2a^pb^q}{n}$. And by Prop. 10. Art. 4. of the Essay, the ratio of ACFH or HDb + ADb to HO is $\frac{1}{n+1}\times\frac{1}{E}$. Wherefore, the sum of these two ratios, or the ratio of 2 HDb to HO, is less than $\frac{1}{n+1}\times\frac{1}{E}+\frac{2a^pb^q}{n}$; and the difference between them, or the ratio of 2 ADb to HO is greater than $\frac{1}{n+1}\times\frac{1}{E}-\frac{2a^pb^q}{n}$. Wherefore, the ratio of 2 HDb to 2 ADb, or that of HDb to ADb, is less than that of $\frac{1}{n+1}\times\frac{1}{E}+\frac{2a^pb^q}{n}$ to $\frac{1}{n+1}\times\frac{1}{E}-\frac{2a^pb^q}{n}$, which is equal to the ratio of $1\times 2Ea^pb^q+\frac{2Ea^pb^q}{n}$ to 1

— 2

$- 2 E a^p b^q - \frac{2 E a^p b^q}{n}$. But the ratio of H D b to A D b, by Art. 6. is greater than the ratio of D b t F to D b f C, when q is greater than p. Wherefore, much more when q is greater than p, the ratio of D b t F to D b f C will be less than that of $1 + 2 E a^p b^q + \frac{2 E a^p b^q}{n}$ to $1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}$. And because, by Art. 8. R b t Q is a mean between D b t F and D b f C, the ratio of D b t F to R b t Q will be less than that of $1 + 2 E a^p b^q + \frac{2 E a^p b^q}{n}$ to $1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}$. And the ratio of D b f C to R b t Q will be greater than that of $1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}$ to $1 + 2 E a^p b^q + \frac{2 E a^p b^q}{n}$.

RULE II.

If nothing is known of an event but that it has happened p times and failed q in $p + q$ or n trials, and q be greater than p; and from hence I judge that the probability of its happening in a single trial lies between $\frac{p}{n}$ and $\frac{p}{n} + z$, (if $m^2 = \frac{n^3}{2pq}$, $a = \frac{p}{n}$, $b = \frac{q}{n}$, E the coefficient of the term in which occurs $a^p b^q$ when $\overline{a + b}|^n$ is expanded, and $\Sigma = \frac{n+1}{n} \times \frac{\sqrt{2pq}}{\sqrt{n}} \times E a^p b^q \times \overline{m z - \frac{m^3 z^3}{3} + \frac{n-2}{2n} \times \frac{m^5 z^5}{5}}$

$n-2$

$\frac{n-2}{2n} \times \frac{n-4}{3n} \times \frac{m^7 z^7}{7} +$ &c.) my chance to be in the right is greater than Σ, and less than $\Sigma \times$

$$\frac{1 + 2 E a^p b^q + \frac{2 E a^p b^q}{n}}{1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}}.$$

For by Art. 11. compared with the value of Σ here set down, the ratio of RbtQ to ACFH is Σ. But by Art. 8. DbtF is greater than RbtQ, and by Art. 12. the ratio of DbtF to RbtQ is less than that of $1 + 2 E a^p b^q + \frac{2 E a^p b^q}{n}$ to $1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}$. From whence it is plain that the ratio of DbtF to ACFH is greater than Σ, and less than $\Sigma \times \frac{1 + 2 E a^p b^q + \frac{2 E a^p b^q}{n}}{1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}}$.

But, as appears from the 10th Proposition in the Essay, the chance that the probability of the event lies between $\frac{p}{n}$ and $\frac{p}{n} + z$ (that is, between the ratio of Ab to AH, and that of At to AH) is the ratio of DbtF to ACFH. Wherefore, the chance I am right in my guess is greater than Σ and less than $\Sigma \times$

$$\frac{1 + 2 E a^p b^q + \frac{2 E a^p b^q}{n}}{1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}}.$$

In like manner, 2dly, the ſame things ſuppoſed, if I judge that the probability of the event lies between $\frac{p}{n}$ and $\frac{p}{n} - z$, my chance to be right is leſs than Σ, and greater than $\Sigma \times \frac{1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}}{1 + 2 E a^p b^q + \frac{2 E a^p b^q}{n}}$. This is manifeſt as the other caſe, becauſe $D h f C$ is leſs than $R h t Q$, but the ratio between them is greater than that of $1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}$ to $1 + 2 E a^p b^q + 2 E a^p b^q$

3dly, If, other things ſuppoſed as before, p is greater than q, and I judge the probability of the event lies between $\frac{p}{n}$ and $\frac{p}{n} + z$, my chance to be right is leſs than Σ, and greater than $\Sigma \times \frac{1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}}{1 + 2 E a^p b^q + \frac{2 E a^p b^q}{n}}$. But if I judge it lies between $\frac{p}{n}$ and $\frac{p}{n} - z$, my chance is greater than Σ, and leſs than $\Sigma \times \frac{1 + 2 E a^p b^q + \frac{2 E a^p b^q}{n}}{1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}}$. And if $p = q$,

which

which ever of these ways I guess, my chance is Σ exactly. This may be proved in the same manner with the foregoing cases, where q is greater than p, or may be proved from them by considering the happening and failing of an event, as the same with the failing and happening of its contrary.

4thly, Other things supposed the same, whether q be greater or less than p, and I judge that the probability of the event lies between $\frac{p}{n} + z$ and $\frac{p}{n} - z$, my chance is greater than $\frac{2\,\Sigma}{1 + 2\,E\,a^p\,b^q + \frac{2\,E\,a^p\,b^q}{n}}$, and less than $\frac{2\,\Sigma}{1 - 2\,E\,a^p\,b^q - \frac{2\,E\,a^p\,b^q}{n}}$. This is an evident corollary from the cases already determined. And here, if $p = q$, my chance is $2\,\Sigma$ exactly.

Thus far I have transcribed Mr. Bayes.

It appears, from the Appendix to the Essay, that the rule here demonstrated, though of great use, does not give the required chance within limits sufficiently narrow. It is therefore necessary to look out for a contraction of these limits; and this, I think, we shall discover by the help of the following deductions; which, for the sake of greater dinstinctness, I shall give as a continuation of the foregoing Articles.

13. The

13. The ratio of the fluxion of $\overline{1-\frac{n^2z^2}{pq}}\Big|^{\frac{n}{2}}$ to the fluxion of $\overline{1+\frac{nz}{p}}\Big|^{p}\times\overline{1-\frac{nz}{q}}\Big|^{q}$ is $\dfrac{\overline{1-\frac{n^2z^2}{pq}}\Big|^{\frac{n}{2}-1}}{\overline{1+\frac{nz}{p}}\Big|^{p-1}\times\overline{1-\frac{nz}{q}}\Big|^{q-1}}$, and the ratio of the fluxion of $\overline{1-\frac{nz}{p}}\Big|^{p}\times\overline{1+\frac{nz}{q}}\Big|^{q}$ to the fluxion of $\overline{1-\frac{n^2z^2}{pq}}\Big|^{\frac{n}{2}}$ is $\dfrac{\overline{1-\frac{nz}{p}}\Big|^{p-1}\times\overline{1+\frac{nz}{q}}\Big|^{q-1}}{\overline{1-\frac{n^2z^2}{pq}}\Big|^{2}}$.

This will immediately appear from Art. 7.

14. While z is increaſing from nothing till $\frac{n^2z^2}{pq}$ becomes equal to unity, theſe two ratios are at firſt greater than the ratio of equality, and increaſe as z increaſes, till they come to a *maximum*. Afterwards they decreaſe untill they become firſt equal to the ratio of equality, and then leſs. This is proved by finding the hyperbolic logarithms of theſe ratios. The hyperbolic logarithm of the firſt is the ſeries $\overline{\frac{q-1}{q}-\frac{p-1}{p}}\times nz+\overline{\frac{q-1}{q^2}+\frac{p-1}{p^2}-\frac{n-2}{pq}}\times\frac{n^2z^2}{2}+\overline{\frac{q-1}{q^3}-\frac{p-1}{p^3}}\times\frac{z^3}{3}+\overline{\frac{q-1}{q^4}+\frac{p-1}{p^4}-\frac{n-2}{p^2q^2}}\times\frac{\quad}{4}+\frac{\quad}{q}$

$-\overline{\frac{p-1}{p^5}} \times \frac{n^5 z^5}{5} + \overline{\frac{q-1}{q^6} + \frac{p-1}{p^6} - \frac{n-2}{p^3 q^3}} \times \frac{n^6 z^6}{6}$ + &c. The hyperbolic logarithm of the second ratio is the series $\overline{\frac{q-1}{q} - \frac{p-1}{p}} \times nz - \overline{\frac{q-1}{q^2}} +$ $\overline{p-1 \qquad \frac{n-2}{pq}} \times \frac{n^2 z^2}{2} + \overline{\frac{q-1}{q^3} - \frac{p-1}{p^3}} \times \frac{n^3 z^3}{3}$ $\frac{q-1}{q^4} + \frac{p-1}{p^4} - \frac{n-2}{p^2 q^2} \times \frac{n^4 z^4}{4}$ + &c. It will appear from examining these two serieses (q all along supposed greater than p) that while z is small the value of each of them is positive, and increases as z increases till it becomes a *maximum*, after which it decreases till it becomes nothing, and after that negative; which demonstrates this article.

15. The former of the two ratios in Art. 13. (q being greater than p) is at first, while z is increasing from nothing, less than the second ratio; and does not become equal to it, till some time after both ratios have been the greatest possible.

Upon considering the two serieses in the last Art. it will appear that the first term of the first series is always positive, the second negative, the third also negative, after which the terms become alternately positive and negative. On the other hand, it will appear that in the second series the two first terms are always positive, and all that follow negative. But as the serieses converge very fast when z is small, the second term being negative in the first series and positive in the second, has a greater effect in making the first series less than the second, than can be compensated for by the terms being afterwards alternately negative and

positive in the one, and all negative in the other. It will further appear from considering these serieses, that the first must continue less than the second 'till z becomes so large as to make the fourth term equal to the second, in which circumstances the two serieses are nearly equal. Afterwards, as z goes on to increase, the value of both lessens continually; but the second now decreasing fastest, as before it increased fastest, becomes first nothing. After which, the other series becomes nothing; and after that both remain negative. From hence it is easy to infer this Article.

16. What has been now shewn of the ratio of the fluxion of $\overline{1-\frac{n^2z^2}{pq}}\Big|^{\frac{n}{2}}$ to the fluxion of $\overline{1+\frac{nz}{p}}\Big|^{p} \times \overline{1-\frac{nz}{q}}\Big|^{q}$ compared with the ratio of the fluxion of $\overline{1-\frac{nz}{p}}\Big|^{p} \times \overline{1+\frac{nz}{q}}\Big|^{q}$ to the fluxion of $\overline{1-\frac{n^2z^2}{pq}}\Big|^{\frac{n}{2}}$ is also true of the ratio of the fluxion of $a^p\,b^q \times \overline{1-\frac{n^2z^2}{pq}}\Big|$ (or Qt in the figure) to the fluxion of $\overline{a+z}|^{p} \times \overline{b-z}|^{q}$ (or Ft) compared with the ratio of the fluxion of $\overline{a-z}|^{p} \times \overline{b+z}|^{q}$ (or Cf) to the fluxion of $a^p b^q \times \overline{1-\frac{n^2z^2}{pq}}\Big|^{\frac{n}{2}}$ or Qt; the latter quantities being only the former multiplied by the common and permanent quantity $a^p\,b^q$. It appears, therefore, that if we conceive Ft, Qt, Cf (Vid. Fig.)

Fig.) to move with equal and uniform velocities, from D b and R b along A H, in order to generate the areas H D b, R W b, A D b; C f will at firſt not only decreaſe faſter than Q t, and Q t than F t; but the ratio of the rate at which C f decreaſes to the rate at which Q t decreaſes, will be greater than the ratio of the rate at which Q t decreaſes to the rate at which F t decreaſes. It appears alſo that after ſome time, firſt C f and Q t, and then Q t and F t will come to decreaſe equally; after which, Q t will decreaſe faſter than C f, and F t faſter than Q t.

17. The curves D F H, R Q W, D C A, have each of them a point of contrary flexure; and the value of z, or of the equal abſciſſes at that point, is in all three $\frac{\sqrt{p q}}{\sqrt{n^3 - n^2}}$. This may be found in the common manner, by putting the ſecond fluxions of the ordinates equal to nothing. In the ſingle caſe, when either p or q is equal to unity, one of theſe points vaniſhes, or coincides with A or H.

18. At the points of contrary flexure (q being greater than p) the ratio of the fluxion of Q t to the fluxion of F t is a *maximum*; and the ſame is true of the ratio of the fluxion of C f to the fluxion of Q t. This is found by making the fluxions of the logarithms of theſe ratios, or of

$$\frac{\overline{p q}|^{\frac{n}{2}-1}}{\overline{1+\frac{n z}{p}}|^{p-1} \times \overline{1-\frac{n z}{q}}|^{q-1}}, \text{ and } \frac{\overline{1-\frac{n z}{p}}|^{p-1} \times \overline{1+\frac{n z}{q}}|^{q-1}}{\overline{1-\frac{n^2 n^2}{p q}}|^{\frac{n}{2}-1}}$$

equal

equal to nothing: which will give the value of z equal to $\frac{\sqrt{pq}}{\sqrt{n^3 - n^2}}$, or the ſame with the value of z at the points of contrary flexure.

19. At the points of contrary flexure, the ratio of the fluxion of C f to the fluxion of Q t, is greateſt in compariſon of the ratio of the fluxion of Q t to the fluxion of F t. This is proved by finding the value of z when the fluxion of the former ratio divided by the latter, or of

$$\frac{\overline{1-\frac{n^2z^2}{p^2}}\Big|^{p-1} \times \overline{1-\frac{n^2z^2}{a^2}}\Big|^{q-1}}{1-\frac{n^2z^2}{pq}}$$

is nothing, which will ſtill give $z = \frac{\sqrt{pq}}{\sqrt{n^3 - n^2}}$. The reaſon, therefore, in the nature of the curve, which, as the ordinates flow, keeps at firſt the exceſs of F t above Q t leſs than the exceſs of Q t above C f, operates with its greateſt force at the points of contrary flexure.

20. The greateſt part of the area R Q W b lies between R b, and the ordinate at the point of contrary flexure. By Art. 11 the ratio of R b t Q to R Q W b is $\frac{n+1}{\sqrt{n}} \times \frac{\sqrt{2}}{2^n} \times G \times \overline{mz - \frac{m^3z^3}{3} + \frac{n-2}{2n} \times \frac{m^5z^5}{5} -}$ &c. Subſtitute here $\sqrt{\frac{pq}{n^3 - n^2}}$ for z, and $\frac{2^n}{\sqrt{nK} \times H}$ * for G (K being the ratio of the quadrantal arc to

* This is always the true value of G; but it would be too tedious to give the demonſtration of this here.

 radius,

radius, and H the ratio whose hyperbolic logarithm is $\frac{3}{12n} - \frac{15}{360n^3} + \frac{63}{1260n^5}$ * &c.) and the ratio of R$b$$t$Q to RQW$b$ at the point of contrary flexure, will be

$$\frac{n+1}{\sqrt{n} \times \sqrt{\overline{n-1}}} \times \frac{.797884}{H} \times \overline{1 - \frac{n}{2.3.\overline{n-1}} + \frac{n \times \overline{n-2}}{2.5.4.\overline{n-1}^2} - \frac{n.\overline{n-2}.\overline{n-4}}{2.3.7.8.\overline{n-1}|^3} + \frac{n.\overline{n-2}.\overline{n-4}.\overline{n-6}}{2.3.4.9.16.\overline{n-1}|^4}} - \&c.$$

Now when n is little, the value of this expression will be considerably greater than .6822. It approaches to this continually as n increases; and when n is large, it may be taken for this exactly. Thus when $n = 6$, this expression is equal to .804. When $n = 110$, it is equal to .6903. If we would know the ratio of R$b$$t$Q to RQW$b$, when C$f$ comes to decrease no faster in respect of Qt, than Qt decreases in respect of Ft; that is, when the excess of Qt above Cf, is greatest in comparison of the excess of Ft above Qt, it may be found (by putting the fourth term of the series in the 14th Art. equal to the second term, and then finding the value of z) to be about .8426, when n, p, and q are considerable; and in other cases greater.

Coroll. 'Tis easy to gather from hence that in like manner the greatest part of the area ADH lies between the two ordinates at the points of contrary flexure †.

* Vid. the Second Rule in the Essay, Phil. Transf. Vol. LIII.

† From this Article may be inferred a method of finding at once, without any labour, whereabouts it is reasonable to judge the probability of an unknown event lies, about which a given number of experiments have been made. For when

21.

21. R*btQ* is greater than the arithmetical mean between D*btF* and D*bfC*. This appears from the latter part of Art. 19. for what is there proved of the ordinates muſt hold true of the contemporary areas generated by them. And though beyond the points at which the ratio of the decreaſe of Q*t* to the decreaſe of F*t* comes to an equality with the ratio of the decreaſe of Q*t* to the decreaſe of C*f*, the exceſs of F*t* above Q*t* begins to grow larger than before in reſpect of the exceſs of Q*t* above C*f*; yet as it appears from the laſt article, that above five ſixths of the areas R Q W *b* and A C F H are generated before the ordinates come to theſe points, and as alſo beyond theſe points the ſaid ratios, 'till they become

neither p nor q are very ſmall, or even not leſs than 10, it will be nearly an equal chance, that the probability of the event lies between $\frac{p}{n} + \frac{\sqrt{pq}}{\sqrt{2n^3 - 2n^2}}$ and $\frac{p}{n} - \frac{\sqrt{pq}}{\sqrt{2n^3 - 2n^2}}$. It will be the odds of two to one that it lies between $\frac{p}{n} + \frac{\sqrt{pq}}{\sqrt{n^3 - n^2}}$ and $\frac{p}{n} - \frac{\sqrt{pq}}{\sqrt{n^3 - n^2}}$; and the odds of five to one that it lies between $\frac{p}{n} + \frac{\sqrt{2pq}}{\sqrt{n^3 - n^2}}$ and $\frac{p}{n} - \frac{\sqrt{2pq}}{\sqrt{n^3 - n^2}}$. For inſtance; when $p = 1000$, $q = 100$, there will be nearly an equal chance, that the probability of the event lies between $\frac{10}{11} + \frac{1}{163}$ and $\frac{10}{11} - \frac{1}{163}$; two to one that it lies between $\frac{10}{11} + \frac{1}{115}$ and $\frac{10}{11} \cdot \frac{1}{115}$; and five to one that it lies between $\frac{10}{11} + \frac{1}{81}$ and $\frac{10}{11} - \frac{1}{81}$.

negative

negative and for some time afterwards, are but small; the effect produced before towards rendering the excess of D $b t$ F above R $b t$ Q always less than the excess of R $b t$ Q above D $b f$ C, will be such as cannot be compensated for afterwards.

A further proof of this will appear from considering that even when R $b t$ Q is increased to R Q W b, it is but little short of the arithmetical mean between A D b and H D b. For from Art. 11. and 20. it may be inferred that the ratio of the whole area R Q W b to this mean, or to $\frac{ACFH}{2}$, is $b \times H$, which is never far from the ratio of equality, but when both p and q are of any considerable magnitude, it is very nearly the ratio of equality. For example; when $n = 110$, $q = 100$, $p = 10$, it is .9938.

22. The ratio of D $b t$ F to R $b t$ Q is less than that of $1 + 2\,E\,a^p b^q + \frac{2\,E\,a^p b^q}{n}$ to one. For by Art. 12. the ratio of D $b t$ F to D $b f$ C is less than that of $1 + 2\,E\,a^p b^q + \frac{2\,E\,a^p b^q}{n}$ to $1 - 2\,E\,a^p b^q - \frac{2\,E\,a^p b^q}{n}$. But by the last Art. R $b t$ Q is greater than the arithmetical mean between D $b t$ F and D $b f$ C, and 1 is exactly the arithmetical mean between $1 + 2\,E\,a^p b^q + \frac{2\,E\,a^p b^q}{n}$ and $1 - 2\,E\,a^p b^q - \frac{2\,E\,a^p b^q}{n}$. From whence this Article is plain.

23. The

23. The ratio of DbtF to ACFH is greater than Σ, and less than $\Sigma \times \overline{1 + 2Ea^pb^q + \frac{2Ea^pb^q}{n}}$. For DbtF being greater than RbtQ, the ratio of it to ACFH must be gieater than the ratio of RbtQ to ACFH, or greater than Σ. Also; since the ratio of RbtQ to ACFH is equal to Σ; and the ratio of DbtF to RbtQ is less than the ratio of $1 + 2Ea^pb^q + \frac{2Ea^pb^q}{n}$ to 1; it follows that the ratio compounded of the ratio of RbtQ to ACFH, and of DbtF to RbtQ, that is, the ratio of DbtF to ACFH must be less than $\Sigma \times \overline{1 + 2Ea^pb^q + 2Ea^pb^q}$.

24. The ratio of DbtF + DbfC to ACFH (that is, the chance for being right in judging that the probability of an event perfectly unknown, which has happened p and failed q times in $p+q$ or n trials, lies somewhere between $\frac{p}{n} + z$ and $\frac{p}{n} - z$) is greater than $\frac{2\Sigma}{1 + 2Ea^pb^q + 2Ea^pb^q}$, and less than 2Σ.

The former part of this Art. has been already proved, Art. 12. The latter pait is evident from Art. 21. For RbtQ being greater than the arithmetical mean between DbtF and DbfC, 2 RbtQ must be greater than DbtF + DbfC; and consequently the

the ratio of 2 R $b t$ Q to A C F H, greater than the ratio of D $b t$ F + D $b f$ C to A C F H*.

It will be eaſily ſeen that this Article improves conſiderably the rule given in Art. 12. But we may determine within ſtill narrower limits whereabouts the required chance muſt lie, as will appear from the following Articles.

25. If c and d ſtand for any two fractions, whenever the fluxion of $c \times Ft$ is greater than the fluxion of $d \times Cf$ (Vid. fig.) $c \times Ft + d \times Cf$ will be greater than Qt. For in the ſame manner with Art. 6. it will appear that $c \times Ft + d \times Cf$ is to Qt, as the fluxion of $c \times Ft \times \overline{1 + \frac{nz}{q}} \times \overline{1 - \frac{nz}{q}}$ together with the fluxion of $d \times Cf \times \overline{1 - \frac{nz}{p}} \times \overline{1 + \frac{nz}{q}}$ to the fluxion of $Qt \times \overline{1 - \frac{n^2 z^2}{pq}}$. Now ſince $1 - \frac{n^2 z^2}{pq}$ is the arithmetical mean between $\overline{1 + \frac{nz}{p}} \times \overline{1 - \frac{nz}{q}}$ and $\overline{1 - \frac{nz}{p}} \times \overline{1 + \frac{nz}{q}}$, it is plain, that were the fluxion of $c \times Ft$ equal to the fluxion of $d \times Cf$, $c \times Ft + d \times Cf$ would decreaſe in reſpect of its own magnitude at the ſame rate with Qt; and, therefore, ſince at firſt equal, they would always continue equal. But the fluxion of $c \times Ft$ being greater than the fluxion of $d \times Cf$ by ſuppoſition, and (ſince q greater than) p $\overline{1 + \frac{nz}{p}} \times \overline{1 - \frac{nz}{q}}$,

* This Art. is true, whether p be greater or leſs than q.

2 following

alſo greater than $1 - \frac{nz}{p} \times \overline{1 + \frac{nz}{q}}$, it follows that the fluxion of $c \times Ft \times \overline{1 + \frac{nz}{p}} \times \overline{1 - \frac{nz}{q}}$ added to the fluxion of $d \times Cf \times 1 - \frac{nz}{p} \times \overline{1 + \frac{nz}{q}}$ is greater than theſe two fluxions multiplied by $1 - \frac{n^2 z^2}{pq}$; and, therefore, greater, than the fluxion of $Qt \times \overline{1 - \frac{n^2 z^2}{pq}}$; and, therefore, $c \times Ft + d \times Cf$ greater than Qt.

26. If we ſuppoſe three continued arithmetical means betwen Cf and Ft $\left(\frac{3Cf + Ft}{4}, \frac{Cf + Ft}{2}, \frac{3Ft + Cf}{4}\right)$ Qt will be greater than the ſecond, and leſs than the third, if p is greater than 1. That Qt will be greater than the ſecond has been already proved; and that it will be leſs than the third, will be an immediate conſequence from the laſt Article, if it can be ſhewn that the fluxion of $\frac{3Ft}{4}$ is greater than the fluxion of $\frac{Cf}{4}$. This will appear in the following manner. The ratio of the fluxion of Cf to the fluxion of Ft is by Art. 7. and 14. $\frac{\overline{1 - \frac{nz}{p}}\Big|^{p-1} \times \overline{1 + \frac{nz}{q}}\Big|^{q-1}}{\overline{1 + \frac{nz}{p}}\Big|^{p-1} \times \overline{1 - \frac{nz}{q}}\Big|^{q-1}}$. The hyperbolic logarithm of

of this ratio is $\overline{\frac{1}{p}-\frac{1}{q}} \times 2nz - \overline{\frac{1}{p^2}-\frac{1}{p^3}-\frac{1}{q^2}+\frac{1}{q^3}} \times \frac{2n^3z^3}{3} - \overline{\frac{1}{p^4}-\frac{1}{p^5}-\frac{1}{q^4}+\frac{1}{q^5}} \times \frac{2n^5z^5}{5}$, &c. This ratio by Art. 18. is greateſt at the point of contrary flexure, or when $z = \frac{\sqrt{pq}}{\sqrt{n^3-n^2}}$. Subſtitute this for z in the ſeries, and it will become $\overline{\frac{1}{p}-\frac{1}{q}} \times \frac{2\sqrt{pq}}{\sqrt{n-1}} - \overline{\frac{1}{p^2}-\frac{1}{p^3}-\frac{1}{q^2}+\frac{1}{q^3}} \times \frac{2p^{\frac{3}{2}}\times q^{\frac{3}{2}}}{3\times\overline{n-1}|^{\frac{3}{2}}}$ –, &c. which, therefore, expreſſes the logarithm of the ratio when greateſt, and will eaſily diſcover it in every caſe. 'Tis apparent that the value of this ſeries is greateſt when p is leaſt in reſpect of q. Suppoſe then $p = 2$, and q infinite. In this caſe, the value of the ſeries will be 1.072, and the number anſwering to this logarithm is not greater than 2.92. The fluxion, therefore, of C f, when greateſt, cannot be three times the contemporary fluxion of F t; from whence it follows that the fluxion of $\frac{3Ft}{4}$ muſt be greater than the fluxion of $\frac{Cf}{4}$.

It is eaſy to ſee how theſe demonſtrations are to be varied when q is leſs than p, and how in this caſe ſimilar concluſions may be drawn. Or, the ſame concluſions will in this caſe immediately appear, by changing p into q and q into p, which will not make any difference in the demonſtrations.

In the manner ſpecified in this Article we may always find within certain limits how near the value of Q t comes to the arithmetical mean between F t and C f, which limits grow narrower and narrower, as

p and q are taken larger, or their ratio comes nearer to that of equality, 'till at laſt, when p and q are either very great or equal, Qt coincides with this mean. Thus, if either p or q is not leſs than 10; that is, in all caſes, where it is not practicable without great difficulty to find the required chance exactly by the firſt rule, Qt will be greater than the fourth, and leſs than the fifth of ſeven arithmetical means between Cf and Ft.

27. The arithmetical means mentioned in the laſt Article may be conceived as ordinates deſcribing areas at the ſame time with Qt; and what has been proved concerning them is true alſo of the areas deſcribed by them compared with $RbtQ$.

28. If either p or q is greater than 1, the true chance that the probability of an unknown event which has happened p times and failed q in $\overline{p+q}$ or n trials, ſhould lie ſomewhere between $\frac{p}{n}+z$ and $\frac{p}{n}-z$ is leſs than $2\,\Sigma$, and greater than $\Sigma + \frac{\Sigma \times \overline{1-2\,E a^p b^q - \frac{2\,E a^p b^q}{n}}}{1+E a^p b^q + \frac{E a^p b^q}{n}}$. If either p or q is greater than 10, this chance is leſs than $2\,\Sigma$, and greater than $\Sigma + \frac{\Sigma \times \overline{1-2\,E a^p b^q - \frac{2\,E a^p b^q}{n}}}{1+\frac{1}{2}\,E a^p b^q + \frac{E a^p b^q}{nn}}$.

This is eaſily proved in the ſame manner with Art. 12, 23, 24.

That it may appear how far what has been now demonſtrated improves the ſolution of the preſent problem, let us take the fifth caſe mentioned in the Appendix to the Eſſay, and enquire what reaſon there is for judging that the probability of an event concerning which nothing is known, but that it has happened 100 times and failed 1000 times in 1100 trials, lies between $\frac{10}{11} + \frac{1}{110}$ and $\frac{10}{11} - \frac{1}{110}$. The ſecond rule as given in Art. 12. informs us, that the chance* for this muſt lie between .6512, (or the odds of 186 to 100) and .7700, (or the odds of 334 to 100). But from the laſt Art. it will appear that the required chance in this caſe muſt lie between $2\,\Sigma$, and

$$\Sigma + \Sigma \times \frac{1 - E\,a^p b^q - \frac{2\,E\,a^p b^q}{n}}{1 + \frac{1}{10} E\,a^p b^q + \frac{E\,a^p b^q}{10\,n}}; \text{ or, between}$$

.6748 and .7057; that is, between the odds of 239 to 100, and 207 to 100.

In all caſes when z is ſmall, and alſo whenever the diſparity between p and q is not great $2\,\Sigma$ is almoſt exactly the true chance required. And I have reaſon to think, that even in all other caſes, $2\,\Sigma$ gives the

* In the Appendix, this chance, as diſcovered by Mr. Bayes's ſecond rule, is given wrong, in conſequence of making m^2 equal to $\frac{n^3}{p\,q}$, whereas it ſhould have been taken equal to $\frac{n^3}{2\,p\,q}$ as appears from Article 8.

true

true chance nearer than within the limits now determined. But not to purſue this ſubject any further; I ſhall only add that the value of 2 Σ may be always calculated very nearly, and without great difficulty; for the approximations to the value of E $a^p b^q$, and of the ſeries $m - z\frac{m^3 z^3}{3} + \frac{n-2}{2n} \times \frac{m^5 z^5}{5}$, &c. * given in the Eſſay, are ſufficiently accurate in all caſes where it is neceſſary to uſe them.

* In the expreſſion for this laſt approximation there is an error of the preſs which ſhould be corrected; for the ſign before the fourth term ſhould be — and not +.

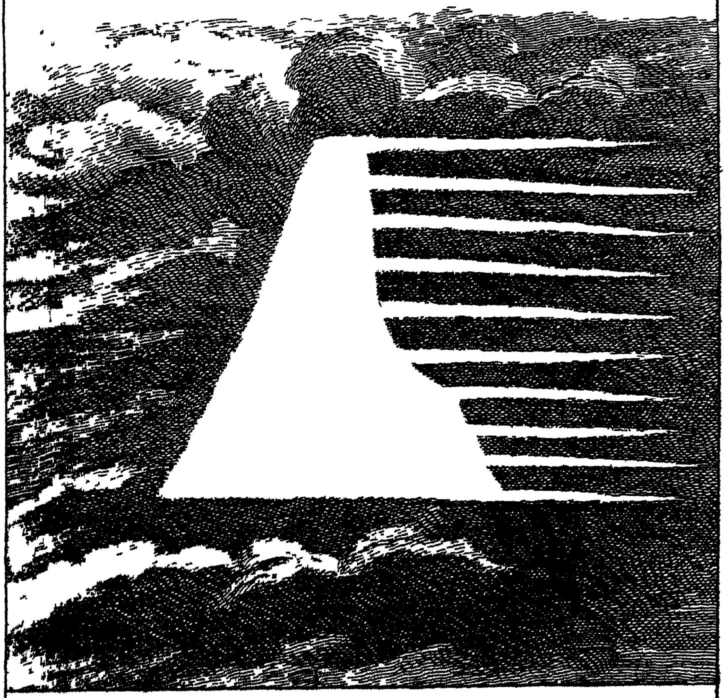
LIII. *An Account of a remarkable Meteor seen at* Oxford, March 5, 1764. *In a Letter to the Rev. Thomas* Birch, *D. D. Secretary to the Royal Society, from the Rev.* John Swinton, *B. D. F. R. S. Member of the Academy* degli Apatisti *at* Florence, *and of the* Etruscan *Academy of* Cortona *in* Tuscany.

Good Sir,

Read Dec. 6, 1764.

COMING out of Christ-Church common-room into the great quadrangle, on Monday, March 5th, 1764, about 7^h $30'$ P. M. I observed, with some surprize, a general brightness in the air, much superior to that of the full moon; though the heavens were then in some measure overcast, and the moon only three days old. This unusual and very remarkable illustration of the atmosphere continued the whole evening, though nothing farther meriting any particular regard (at least nothing that I either saw or heard of) for two or three hours occurred. But throwing up my bedchamber sash, a little before eleven o'clock, I unexpectedly discovered a most glorious and exceedingly resplendent white [TAB. XVIII.] column in the southern part of the hemisphere, which in lustre surpassed every thing of the same kind that I had ever seen before. The base of this column seemed to be between twenty and thirty degrees distant from the horizon, and was many degrees broad. The meteor ascended gradually near thirty degrees, passing to the S. of the

zenith.



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zenith. It was much narrower at the vertex than the baſe, and conſequently approached ſomewhat towards a pyramidal form. It remained a few minutes in a fixed and permanent ſtate, after it had arrived at its greateſt altitude, and was completely formed. About 11^h $15'$ P. M. it grew fainter, and much leſs vivid; and there then darted from it towards the W. ſeveral whitiſh rays and coruſcations. At 11^h $20'$ the lucid column was barely viſible, declining apparently ſouthward, and ſoon after totally diſappeared. I went to bed at 11^h $30'$ P. M. when the atmoſphere was covered with the ſame kind of luminous vapour, that before the formation of the bright Coloſſean pillar had appeared; and, in the Southern part of the hemiſphere, diverſified by undulations of ſhining matter, that exhibited a moſt beautiful and agreeable ſcene.

It may not be improper here to remark, that a meteor, called an *Aurora Borealis*, was ſeen at Liſbon, according to * one of the public papers, the very ſame night. It is ſaid to have laſted about four hours, and to have engaged the attention of the philoſophers there. As from the ſimilarity of certain circumſtances it might have been denominated an *Aurora Borealis*, though appearing in the ſouthern part of the heavens, as that I obſerved actually did; a more particular and diſtinct account of this phænomenon might poſſibly enable us to determine, with ſome degree of probability, whether or no it was the ſame with that by me here ſo imperfectly deſcribed. Could the identity of theſe meteors be clearly evinced, or indeed rendered probable, ſeveral curious corolla-

* LLOYD's *Evening Poſt*.

ries,

ries, relative to the altitude, motion, velocity, &c. and even the very nature itſelf, of that I had ſo tranſient a view of, might perhaps be deducible from it.

Inſtances of *Auroræ Auſtrales*, at leaſt in our part of the world, are immenſely rare. At preſent that obſerved by John Martyn, M. D. F. R. S. and Profeſſor of Botany in the Univerſity of Cambridge, only occurs. The account of this very uncommon appearance, tranſmitted by that ingenious gentleman to the Royal Society, and publiſhed by them in the *Philoſophical Tranſactions* *, highly merits the attention of the curious meteorologiſt. This phænomenon, ſeen by him, January 23, 1749-50, and that of which I now ſend you ſo imperfect a deſcription in ſeveral reſpects pretty well agreed; but in others, which it would be ſuperfluous even to touch upon here, almoſt totally differed. Some of the † public papers informed us, that an extraordinary phænomenon was obſerved in the air at London, the preceding night, viz. March 4, 1764; which in a few particulars reſembled that of which I have taken the liberty to communicate to you a ſhort account in this letter, but in the reſt thoſe two meteors were diſſimilar enough. Perhaps theſe *Auroræ Auſtrales* may ſome time or other hereafter be more frequent than hitherto they have been, and by certain new circumſtances attending them farther elucidate the theory of this ſpecies of meteors; the true cauſe of which, notwithſtanding the labours and reſearches of ſeveral

* *Philoſ. Tranſact.* Vol. XLVI. p. 319.

† *The* St. JAMES's *Chronicle*, *&c.* N° 468.

learned men, ſeems not yet to be perfectly known. I am, with all poſſible conſideration and eſteem,

S I R,

Your much obliged,

and moſt obedient ſervant,

Chriſt-Church, Oxon.
Aug. 22, 1764.

John Swinton.

LIV. *Extract of a Letter from Mr.* John Horſley, *Fourth Mate on board the* Glatton Eaſt-India *Ship, to the Rev. Mr.* Nevil Maſkelyne, *F. R. S. dated* Batavia, Nov. 16, 1763, *giving an Account of his Obſervations, at Sea, for finding out the Longitude by the Moon.*

Dear Sir;

Read Dec. 13, 1764.

YOU was ſo good as to expreſs a deſire of hearing from me, by every opportunity, during the time of my voyage; a requeſt that I ſhall always comply with, with a great deal of pleaſure.

I have the misfortune to inform you of our having loſt our paſſage to China, the original occaſion of which was our late departure from England. We

arrived at Bencoolen July 29, where we were detained till the 13th of September. We found the winds ſtrong ſet againſt us in the China ſeas, which obliged us to bear away for this place.

I ſhall now give you an account of the obſervations I have made, ſince I have been out, and the ſucceſs attending them.

We ſailed from Spithead March the 8th, 1763; the 19th I had four obſervations of the diſtance of the Moon from the Sun; by taking the medium the longitude agreed exactly with that by account. The 21ſt I had another obſervation, and, the ſame day, ſaw the iſland of Madeira, the body of which, according to this and the former obſervations (they agreeing exactly) I made to lie 17° 18′ weſt of London, which differs only 18 minutes from what it is laid down in the chart. The ſucceſs I met with in this firſt attempt gave me great ſatisfaction, and made me continue my obſervations regularly to the iſland of St. Paul's, which we made July 5. The day before I had three obſervations of the diſtance of the Moon from the Sun. July the 5th, the body of the iſland bearing by the azimuth compaſs S. 27° W. diſtance ſix leagues, the ſky remarkably clear and fine, and the ſhip having hardly any motion, circumſtances all in my favour, I took nine obſervations of the diſtance of the Moon from the Sun, the captain and chief mate aſſiſting me in taking the altitudes. I divided them into three ſetts, and worked from the medium of every three; by which I made the longitude of the ſhip as follows, 75° 15′, 75° 25′, 74° 40′. The three obſervations, I took the day before, made the longitude of the ſhip 74° 38′ and 73° 32′ which brought

brought forward to the noon of July the 5th made 75° 45′ and 74° 39′. Taking the medium of the whole five ſetts I made the longitude of the ſhip at noon 75° 8′ 48″ eaſt of London. Subſtracting from thence the difference of longitude, the bearings and diſtance of the iſland gave = 8′ 37″ weſt, I made the longitude of St. Paul's 75° 0′ 11″ eaſt of London, and 58° 0′ 11″ from the Cape of Good Hope. By my account kept from an obſervation taken June the 18th, I made it 73° 35′ eaſt of London, and 56° 35′ from the Cape, which differs 1° 25′ from what I make the true longitude: moſt of the accounts on board were between two and three degrees to the weſtward of mine. The longitude of this iſland having never been determined by any other method than the runs of ſhips to the Cape, there are hardly any two charts or books that lay it down alike, they differing from 71° to 74° in their accounts, which made me put little dependance upon any of them.

On our arrival at Bencoolen I took three obſervations of the diſtance of the Moon from the Sun, in the road, by which I made Fort Marlborough to lie in 103° 50′ 45″ eaſt of London.

I was on ſhore five or ſix days in hopes of getting ſome obſervations of Jupiter's Satellites, but was diſappointed by the cloudineſs of the nights; ſo that I got nothing for my pains but a fever, which had nigh coſt me my life, terminating at laſt in an intermiting one, which has continued with me ever ſince, neither does it ſeem to have any inclination to leave me at preſent.

I have ſaved all the obſervations I have made, and the work of them, which I ſhould have ſent you a complete copy of, if I had been well enough to have tranſcribed them.

I am,

Yours, &c.

John Horſley.

" Mr, Horſley, whoſe ſkill and diligence are bet-
" ter evinced by his own account than by any enco-
" miums I can give them, made uſe of a quadrant
" made by Mr. Bird, and my Britiſh Mariner's
" Guide, for determining the longitude of the ſhip
" at ſea.

N. Maſkelyne.

LV. *An Account of a remarkable Meteor ſeen at* Oxford, April 23, 1764. *In a Letter to the Rev.* Thomas Birch, *D. D. Secretary to the Royal Society, from the Rev.* John Swinton, *B. D. F. R. S. Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany.

Good Sir;

Read Dec. 13, 1764.

HAVING taken a turn on the Parks, or Public Univerſity-walk here, on Monday April 23, 1764, towards the decline of the afternoon; I made a viſit to a friend in town, with whom

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whom I have now and then an article of bufinefs to tranfact. Upon my return home, about 8^h $10'$ P. M. looking over the houfes oppofite to Alban-Hall; I obferved a very remarkable kind of light, forming the reprefentation of an exceeding bright crepufculum, or expanded body of vapour, which diffufed itfelf over all the northern part of the hemifphere that prefented itfelf to my view. This I looked upon as a prelude to an *Aurora Borealis*, in fome form or other. But as fuch appearances are pretty common here, efpecially of late years, I then paid no great attention to it. About 8^h $55'$, not thinking of what I had feen, I threw up my fafh, and accidentally caft my eye towards the N. W. where, to my very great furprize, I difcovered a luminous arch, [TAB. XIX.] extending itfelf to the oppofite part of the heavens, fomewhat refembling an *Iris*, but of a bright white colour. I then went out into the ftreet, traverfed part of the town, and found the arch both in the N. W. and S. E. to be nearly terminated by the horizon; fo that it feemed to be almoft perfectly femicircular, and confequently in a manner to biffect the hemifphere, when completely formed. The meteor was not exactly erect, but afcended obliquely, declining a little to the N. of the zenith. It was extremely narrow, in breadth fcarce exceeding two degrees. It's edges towards the S. E. were not fo well defined, but fomewhat jagged and unequal. From 9^h to 9^h $15'$ it exhibited a moft vivid refplendent whitenefs, fuch as, I believe, was hardly ever obferved before. During that term, the phænomenon feemed altogether fixed and permanent, without increafe or diminution, without any apparent motion

of

of the whole, and indeed almoſt without the leaſt external variation. An internal undulating motion of the particles conſtituting the white luminous matter of the arch was nevertheleſs diſcernible, from the firſt to the laſt moment of it's exiſtence. No ſtars were viſible through the vapour itſelf, but two or three appeared at a ſmall diſtance from it. Theſe, however, were much obſcured by the interpoſition of ſome thin whitiſh clouds, with which that part of the atmoſphere was at this time covered. Not the fainteſt traces of a proper *Aurora Borealis*, either before the firſt appearance, during the continuance, or after the extinction of the meteor, were to be ſeen. Several young people were viewing it, when I went into the ſtreet; who ſeemed, according to cuſtom, not a little alarmed at ſo unuſual a ſight. One of them told me, that the arch began to be formed about a quarter before nine. In other parts of the city this wonderful phænomenon was likewiſe obſerved, both by townſmen and members of the Univerſity, not without ſome degree of aſtoniſhment and ſurprize. A little paſt nine o'clock the extremities of the arch grew faint, as did ſoon after the whole body of the luminous vapour itſelf. About $9^h\ 20'$ the ſummit, or higheſt part, of the arch, a few degrees to the N. of the zenith, only remained; which continued gradually decreaſing 'till $9^h\ 27'$, when the whole totally diſappeared.

With regard to the weather, the morning of the 23d was dark and lowring; but the remainder of the day, from $10^h\ 45'$ A. M. to ſun-ſet, was bright and clear, though cold out of the ſun. The wind 'till ſix P. M. was northerly, and blew pretty freſh,

 but

but then came about to the N. W. From that time to 8h 15' P. M. it gradually decreaſed, and was ſucceeded by almoſt a perfect calm, the leaſt breath of air being then ſcarce perceptible. During the continuance of the meteor, this calm remained; and after the extinction of it, the weather was conſiderably milder than before. The 24th was a warmer day than any we had had ſince the month commenced, the ſun irradiating us from morning 'till evening with his ſalutary rays. The whole hemiſphere and the horizon this evening were clear and ſerene, the firmament being but ſlightly interſperſed with thin whitiſh clouds. That part of it near the horizon was tinged with a moſt beautiful red colour. The ſun, juſt before he emerged out of our hemiſphere, perfectly reſembled a globe of fire.

I have not yet been able to meet with an inſtance of a ſimilar phænomenon in any phyſiological papers, publiſhed before the year 1750. But accounts of two or three meteors ſomewhat reſembling that above deſcribed, in our *Philoſophical Tranſactions* *, then occurred. However, that of the 23d of April, 1764, differed from one of theſe in it's extent, as well as the inconſiderable breadth of the zone forming the arch, and the biſſection of the hemiſphere. From the others it was ſufficiently diſtinguiſhed by it's moſt vivid reſplendent whiteneſs, without any ſhort, white, vibrating columns attached to it; eſpecially, as it was neither preceded, attended, nor followed by any ſtreaming luminous rays, or coruſcations. I cannot help therefore conſidering this as a ſingular

* *Philoſoph. Tranſact.* Vol. XLVI. p. 345. 346, 347, 648, 649.

ſort

ſort of phænomenon, never hitherto honoured with an adequate deſcription. If it ſhould appear to the Royal Society in the ſame light, they will excuſe the trouble given on this occaſion by,

SIR,

Your much obliged,

and moſt obedient,

humble ſervant,

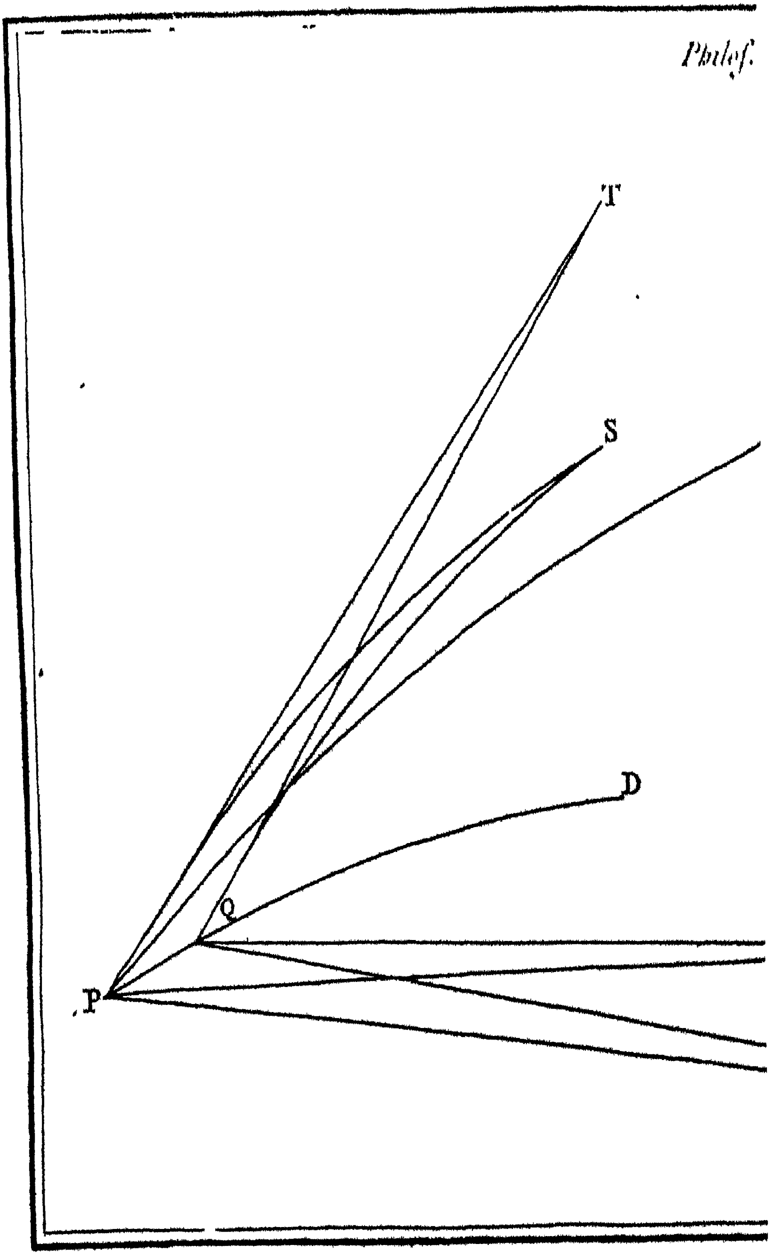
Chriſt-Church, Oxon.
Aug. 29, 1764.

John Swinton.

LVI. *Some Remarks upon the Equation of Time, and the true Manner of computing it. By* Nevil Maſkelyne, *A. M. Fellow of* Trinity College, Cambridge, *and F. R. S.*

Read Dec. 13, 1764.

THESE remarks were wrote above a twelve-month ago, and would have been then communicated to the Royal Society, had not my voyage to Barbados prevented it. Since my return from thence, I find part of the miſtakes here pointed out acknowledged and corrected by M. Delalande, in his Treatiſe of Aſtronomy lately publiſhed, to whom I remember to have communicated my ideas on the ſubject, when he was in England. Nevertheleſs, as the error ariſing from taking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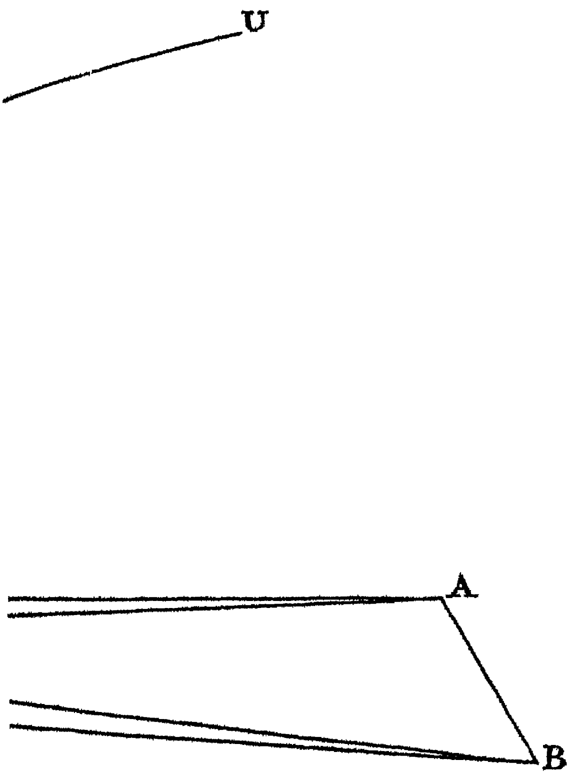

J. Mynde sc.

taking the equation of the equinoctial points into the account ſtill remains uncorrected by him; and as I flatter myſelf, that what is ſaid here may tend to ſet the whole matter in a clear light, I apprehend the publication of theſe remarks may ſtill be proper.

The French Almanack, called the Connoiſſance des Mouvements Celeſtes, hath been deſervedly eſteemed by aſtronomers, as the moſt complete publication of its kind. Its preſent learned editor M. Delalande hath rendered its uſe more extenſive by making the calculations from the lateſt and moſt approved tables, and alſo adding ſuch explanations of them, as, at the ſame time, lay open before his readers the moſt conſiderable improvements of modern aſtronomy. Neverthelefs, as the beſt mathematicians are not infallible, ſo I have reaſon to think I have diſcovered ſome errors in M. Delalande's method of computing the equation of time in this ephemeris, or, which comes to the ſame thing, the mean time, at the inſtant of apparent noon.

M. Delalande ſays, page of the Connoiſſance for 1760, which he repeats in the publications of other years, that, "to calculate exactly the difference between "mean and true time (that is to ſay the equation of "time) at the inſtant of apparent noon, the ſum of "the equation of the ſun's centre, the difference "between his longitude and right aſcenſion, the lunar "equation, the equations of Jupiter and Venus, and "that of the preceſſon of the equinoxes, with their "proper ſigns, muſt be converted into mean ſolar time. "He adds, that it was impoſſible, before this time, "to obtain the equation of time exactly; 1ſt, becauſe "hitherto no account has been made of the four

" little equations, the sum of which may produce
" above three seconds of time; 2dly, because it has
" been the practice to convert the equation of the
" sun's centre, and the difference between his right
" ascension and longitude into time of the Primum
" Mobile, instead of converting them into mean
" solar time, which, says he, may produce an error
" of two seconds and a half; 3dly, because the
" equation of the sun's centre was not known ex-
" actly before, every minute of which answers to
" four seconds in the equation of time."

I readily agree with M. Delalande, that the equation of time could not be had so exactly formerly, as it may now, when we have a much more exact theory of the sun, and are lately made acquainted with new equations of his motion. I cannot, however, assent to his position, that the equation of the equinoctial points is to be taken into this account, together with the other equations, since this is not an inequality in the sun's motion, but arises from a motion of the equator itself; yet of such a kind as cannot accelerate or retard the coming of the sun, or any star lying within the tropics, to the meridian, by above a quarter of a second of time. This will, perhaps, appear in a good measure plain, if it be considered, that the diurnal motion of the earth round its axis is neither accelerated nor retarded by the action of the sun and moon in producing the precession of the equinoxes, and variations of the inclination of the earth's axis to the ecliptic. The effect of these actions is, that the terrestrial pole, each day, describes a small arc of a circle about the centre of the earth, in the plane of a celestial meridian passing through

through the ſun or moon, or rather one between both; and, conſquently, the equator of the earth has its motion in its own plane neither accelerated nor retarded, but obtains a new motion, wnoſe axis is one of its own diameters. This is the true origin, as well of the minuter and periodical nutations, as of the regular and perpetual motion of the earth's axis about the pole of the ecliptic, obſerved in all ages, on which the continual pieceſſion of the equinoxes depends.

But, to illuſtrate more fully the point in queſtion, let P, ſee fig. repreſent the north pole of the celeſtial equator, which ſuppoſe to be tranſlated, in any certain time, from P to Q, through the ſmall ſpace P Q, upon the meridian P D, by the actions of the ſun and moon; let A be the equinoctial point of aries, and S the ſun or ſtar. It is evident, that, as the rotation of the earth round its axis is no way affected, the tranſlation of the celeſtial pole from P to Q along the arch P Q, of the celeſtial meridian P D, will occaſion no alteration in the time of any given meridian of the earth coming to the fixt celeſtial meridian P D, nor conſequently in the time of the ſun or ſtars, when lying in this meridian, appearing to paſs the meridian of the given place; contrary to what ſhould follow from the method of computing the equation of time, uſed in the Connoiſſance des Mouvements Celeſtes; according to which, as long as the equation of the equinoxes is any thing, the equation of time muſt be affected thereby, and conſequently the abſolute time of the ſun's paſſing the meridian.

 But,

But, if the ſun or ſtar lie not in the celeſtial meridian P D, but in ſome other meridian P S, at S, then the ſpherical angle S P D is the diſtance of the ſun from the meridian P D, when the pole is at P, and S Q D is his diſtance from the ſame meridian, when the pole is tranſlated to Q. Let P T, Q T, meeting in T, be tangents of the meridians P S, Q S, in P and Q; T Q D being the external angle of the rectilineal triangle T P Q, the angle P T Q is = T Q D — T P D = S Q D — S P D, and, therefore, is a meaſure of the alteration of the time of any meridian of the earth's coming to the ſun at S, produced by the tranſlation of the pole from P to Q. Now the ſine of P T Q is to the ſine of T P Q, as P Q to T Q; whence, calling the radius unity, and taking P Q, on account of its ſmallneſs, = the ſine of P Q, and the angle P T Q = the ſine of P T Q, we have $P\,T\,Q = \frac{P\,Q \times \text{ſine } T\,P\,Q}{T\,Q}$ = the tranſlation of the pole × the ſine of the right aſcenſion of the ſun or ſtar reckoned from the meridian in which the pole moves, ÷ the tangent of the polar diſtance, or, which is the ſame thing, × the tangent of the declination. Therefore, as P Q, ariſing from the nutation of the earth's axis, never exceeds $9'' \frac{1}{2}$, the greateſt value of P T Q, for the ſun can never exceed $9'' \frac{1}{2} \times$ tangent of $23° \frac{1}{2}$ the ſun's greateſt declination, = 4″, 1 which anſwers to about $\frac{1}{4}$ of a ſecond of time: and ſo much, and no more, may the ſun come ſooner or later to the meridian, on account of the nutation of the earth's axis: whereas, if the equation of the equinoxes was to be applied directly in the computation, according to M. Delalande's

M. Delalande's method, it would ſometimes, namely when at its maximum of 18″, produce nearly 1 $\frac{1}{4}$ ſecond of time.

But, tho' this demonſtration may be admitted to be juſt, yet it may perhaps be aſked, wherein lay the fault of the method of computation here cenſured, and whether the time of the ſun's coming to the meridian is not regulated by his right aſcenſion? It may alſo be thought requiſite, that the true manner of computing the equation of time, from the ſun's right aſcenſion, ſhould be ſhewn.

Firſt, let it be obſerved, that when the pole is at P, A is the equinoctial point, and, when the pole is tranſlated to Q, ſome other point B is the equinoctial point: therefore the ſun's mean right aſcenſion U P A is reckoned from A, and his apparent right aſcenſion B Q S, computed from his longitude, corrected by the equation of the equinoxes A B, or B S, is reckoned from another point B. Now the equation of time is proportional to the difference between the ſun's mean and true right aſcenſion, both reckoned from the ſame point; ſo that if the ſun's mean right aſcenſion is reckoned from A, his apparent right aſcenſion, in this caſe, ſhould be reckoned from A too; or if the apparent right aſcenſion is reckoned, more properly, from the apparent right equinox B, his mean right aſcenſion, for this purpoſe, ſhould be reckoned from B likewiſe. For it is plain, from what has been ſaid above, that no ſmall motion of the pole P can at all affect the abſolute time of a ſtar in the equator's coming to the meridian of any place; for, the tangent Q T then becoming infinite, the angle P T Q vaniſhes; therefore the mean equi-

nox

nox A will come to the meridian at the ſame inſtant of abſolute time, as if the pole had not been tranſlated from P to Q; and the difference of time between the ſun S coming to the meridian, and a fictitious ſun U, ſuppoſed to move uniformly in the equator, with a motion equal to the ſun's mean motion in longitude, or the equation of time will be therefore meaſured by A Q S ∽ A P U, the difference of their right aſcenſions reckoned from the ſame point A. It will alſo, by the like reaſoning, be meaſured by B Q S ∽ B P U, the difference of their right aſcenſions reckoned from the ſame point B; for B being the equinox, when the pole is at Q, the abſolute time of the point B paſſing the meridian of any place will remain the ſame as if the pole had continued at P; whence the propoſition eaſily follows, in like manner as above.

It may be now proper to ſhew how the equation of time, as affected by the nutation of the earth's axis, ought to be computed. This may be done two ways. The "firſt follows from what has been juſt
" laid down: correct the mean right aſcenſion of
" the ſun U P A, by the preceſſion of the equinoxes
" in right aſcenſion A P B (which is always to the
" preceſſion in longitude B A, as coſine of the obli-
" quity of the ecliptic, to the radius, or as 12 to
" 13 nearly) the difference of the ſun's mean right
" aſcenſion thus corrected BPU, and the ſun's appa-
" rent aſcenſion B Q S, turned into time, is the true
" equation of time."

Otherwiſe the effect of the nutation of the earth's axis upon the equation of time, if thought deſerving notice, as it can never exceed $\frac{1}{4}$ of a ſecond of time,

time, might be computed from the angle P T Q = P Q × fine of T P D ÷ T Q, which, fuppofing the nutation of the pole to be performed in a circle, whofe radius is 8″, or a mean between the two conjugate femi-axes of the ellipfis, in which it really moves, is = 8″ × tangent of the fun's declination × cofine of the difference of fun's right afcenfion, and the longitude of the moon's afcending node.

But this is not the only miftake in the computation of the equation of time in the Connoiffance des Mouvements Celeftes, though it may exceed one fecond of time. M. Delalande fays that the fum of the equation of the fun's centre, the difference between his longitude and right afcenfion, and the fum of the four little equations, muft be converted into mean folar time, in order to find the equation of time; and adds, that no exact equation table could be had, before this time, for three reafons, one of which is, that it has always been the practice to convert the equation of the fun's centre and the difference between his longitude and right afcenfion into time of the Primum Mobile, inftead of mean folar time, which, fays he, may produce an error of 2 $\frac{1}{2}$ feconds.

Now I muft here freely own, that as I could not, without fome reluctance, and only from the fulleft proof, allow all the mathematicians and aftronomers, before this time, to have been miftaken in the manner of converting the quantities above-mentioned into time, fo I can find no reafon to conclude fo from what has been cited above: on the contrary, from a full confideration of the fubject, I apprehend the method hitherto ufed by the mathematicians to be

be juſt, and that the author has himſelf fallen into an equal miſtake with that of which he accuſes them.

But, in order to ſet this matter in a clearer light, it will be firſt neceſſary to conſider motion and time, relatively to each other; for, except this be done, it will be impoſſible to underſtand any thing preciſe from converting a certain number of minutes and ſeconds into mean ſolar time, or time of the Primum Mobile.

There are three different kinds of time uſed by aſtronomers, ſidereal time, apparent ſolar time, and mean ſolar time. The interval between the tranſit of the firſt of Aries acroſs the meridian one day, and its return to it the next day, is called a ſidereal day, which is divided into 24 equal parts or hours, and the hours into minutes, &c. This time is ſhewn by a clock regulated to agree with the tranſit of the ſtars acroſs the meridian. The interval between the tranſit of the ſun acroſs the meridian one day, and his tranſit the next day, is called an apparent ſolar day, which is divided into hours, minutes, &c. of apparent time. The ſolar day, it is manifeſt, and its hours, minutes, &c. are of different lengths, at different times of the year: on account of which inequality, a good clock, which keeps equal time, cannot long agree with the ſun's motion, which is unequal. Therefore, aſtronomers have deviſed an imaginary time, called mean ſolar time; which is what would be pointed out by the ſun, if his motion in right aſcenſion from day to day was uniform, or, in other words, it is what would be pointed out by a fictitious ſun or planet ſuppoſed to move uniformly in the equator, with a motion equal to the mean

motion of the ſun in longitude, its diſtance from the firſt point of Aries (meaning hereby the mean equinox) being always equal to the mean longitude of the ſun: and as apparent noon is the inſtant of the true ſun's coming to the meridian, ſo mean noon is the inſtant at which this fictitious planet would come to the meridian. The interval between its coming to the meridian on any two ſucceſſive days is a mean ſolar day, which is divided into hours, minutes, &c. of mean ſolar time; all which it is manifeſt will preſerve the ſame length at all times of the year.

The equation of time, at the inſtant of apparent noon, or of the ſun's paſſing the meridian, being equal to the difference between mean time and 12 hours, is alſo equal to the interval between the mean and true ſun's paſſing the meridian expreſſed in mean ſolar time: to find which, we have the diſtance of the mean ſun from the meridian, at the inſtant of apparent noon, equal to the difference between the ſun's apparent and mean right aſcenſion (both reckoned either from the mean or apparent equinox) which may be called the equation of right aſcenſion. The queſtion, therefore, comes to this, How many minutes and ſeconds of mean ſolar time doth the mean ſun take to move this diſtance up to or from the meridian? Aſtronomers hitherto have allowed 1 minute of time to every 15 minutes of right aſcenſion, and ſo in proportion; and, I apprehend, juſtly too; for does not the mean ſun, in returning to the meridian, deſcribe 360° about the pole in 24 hours of mean ſolar time; whence it is plain, that his departure from the meridian is at the rate of 15° to 1 hour, and 15′ to 1 minute of mean ſolar time.

 Therefore

Therefore aſtronomers have not converted the equation of right aſcenſion into time according to the motion of the Primum Mobile; for, the equation of time being mean ſolar time, and the motion of the Primum Mobile being compleated in 23 H. 56 M. 4 S. of mean ſolar time, therefore 15° motion of the Primum Mobile does not anſwer to 1 hour of mean ſolar time (though it does to 1 hour of ſidereal time) but to the 24th part of 23 H. 56 M. 4 S. or 59 M. 50 $\frac{1}{6}$ S. And it appears, that the equation of time in the Connoiſſance des Mouvements Celeſtes has been computed in this manner, and the table in the 79th page of the Connoiſſance for 1761 has been made uſe of, entitled, "A table to convert into degrees the time of a clock regulated according to the mean motion of the ſun." The degrees of this table are evidently degrees of the Primum Mobile, 1 hour of mean ſolar time giving 15° 2′ 27,8″, which anſwers to the motion of the ſtars from the meridian, but not to the mean motion of the ſun from thence, which is 15° to 1 hour of mean ſolar time: whence it appears, that this writer hath evidently fell into the miſtake of taking motion or ſpace of the Primum Mobile, inſtead of the mean motion of the ſun from the meridian; an equal miſtake to that of which he erroneouſly ſuppoſes former mathematicians to have been guilty, in computing the equation of time. So that the equation of time in this ephemeris, beſides the miſtake ariſing from the taking in the equation of the equinoctial points into the account, is conſtantly too ſmall in the proportion of 24 hours to 23 H. 56 M. 4 S. or of 366 to 365, or too ſmall by 1 ſecond upon every 6 minutes of the equation

equation of time: and the miſtake of 2 $\frac{1}{2}$ ſeconds, which was ſuppoſed to be found in the old manner of reducing the equation of right aſcenſion into time, really takes place in this new method; which, added to 1 ſecond of time, ariſing from the miſtake in taking the preceſſion of the equinoxes into the account, produces 3 $\frac{1}{2}$ ſeconds, an error which, I apprehend, the aſtronomical equation tables uſed ſince Mr. Flamſtead's time have but rarely exceeded.

To ſome, who are not well acquainted with the preſent improved ſtate of aſtronomy, the difference in queſtion may ſeem a matter of indifference, and too trifling for notice. But, if truth is the object of all our enquiries, why ſhould we wilfully go beſide it in the ſmalleſt matters? And is it not a juſtice due to paſt aſtronomers, to whom we owe the foundations of all our knowledge, to vindicate them even from the ſmalleſt cenſure, which they do not appear to deſerve? At the ſame time, I flatter myſelf, that the learned editor of the Connoiſſance des Mouvements Celeſtes, and alſo the friends of the late illuſtrious Abbé de la Caille, who, I believe, was inadvertently the firſt author of this miſtake, will take no offence at my endeavouring to clear up a point, which they, doubtleſs for want of having given ſufficient attention to, ſeem to have miſtaken: ſince, truth being the common object of all our purſuits, we ought candidly to accept as well the aſſiſtance we receive from each other for bringing us into the right road, when we happen to have ſtrayed from it, as for helping us forward on our journey.

The Figure referred to in p. 339. *ſhould be* TAB. XX.

LVII. *Aſtronomical Obſervations made at the Iſland of St.* Helena, *by* Nevil Maſkelyne, *M. A. Fellow of* Trinity College, Cambridge, *and F. R. S.*

TO

THE RIGHT HONOURABLE

The Earl of MORTON, PRESIDENT,

AND

The FELLOWS of the ROYAL SOCIETY,

THE

Following Obſervations made, when I was employed, by their Appointment, at ST. HELENA,

ARE

Moſt reſpectfully preſented, by,

His Lordſhip's,

And the Royal Society's,

Moſt obedient,

Humble Servant,

Nevil Maſkelyne.

THE

Read Dec. 20, 1764, and Jan. 20, 1765.

THE following obſervations were taken with a reflecting teleſcope, of two feet focal length, made by Mr. Short (of a ſimilar ſize and conſtruction to thoſe uſed in the obſervation of the Tranſit of Venus, by himſelf at Saville Houſe, by Mr. Green at Greenwich, and by Meſſ. Maſon and Dixon at the Cape of Good Hope), with an equal altitude inſtrument made by Mr. Bird, and a clock, with a gridiron pendulum, made by Mr. Shelton, an account of whoſe going, at Greenwich, before my departure for St. Helena, and immediately upon my arrival there, is contained in Phil. Tranſ. Vol. LII. Part II. Page 434. and the difference of gravity between thoſe two places thence deduced.

The almoſt continual cloudineſs of the ſkies, at the Iſland of St. Helena, renders it a very inconvenient place for the making of aſtronomical obſervations, which I had the mortification to experience in loſing the ſight of the exit of the planet Venus, from the ſun's diſc, on the 6th of June 1761, to obſerve which was the primary motive of my going thither. I ſhould have thought myſelf, in a great meaſure, compenſated for this miſchance, if I had been enabled, by the help of the ten foot ſector, provided me at the expence of the Royal Society, either to prove or diſprove the exiſtence of a ſenſible annual parallax of the ſtar Sirius, ſome reaſons for the probability of which I laid before the Royal Society, in a paper ſince publiſhed in Phil. Tranſ. Vol. LI. P. II. p. 889, and, at the

the ſame time, offered a propoſal for the diſcovery of the ſame, by obſervations of the zenith diſtance of that ſtar, to be made at the iſland of St. Helena. But, unfortunately, when I came to ſet up the ſector there (which, through the tardineſs of the workman in finiſhing it, I had not had an opportunity of proving, as I had wiſhed, before my departure from England) I ſoon found a ſtrange irregularity in the obſerved zenith diſtances of the ſtars, amounting to 10, 20, and ſometimes even 30 ſeconds. After having ſatisfied myſelf, by various trials, that theſe great differences in the obſervations did not ariſe from any bending of the tube of the teleſcope, which conſtitutes the radius of the inſtrument, or from any looſeneſs in the object-glaſs, or inſtability of the wooden three-legged ſtand, which ſupports the ſector, I, at laſt, found the cauſe of error to lie, where I had leaſt ſuſpected it, in the imperfection of the ſuſpenſion of the plumb-line (which is a fine ſilver wire) from the neck of the central pin; for, upon taking the loop of the plumb-line off the pin, and putting on again, after turning it half round, or putting on a new one, I found the plumb-line would apply itſelf to a different part of the limb of the ſector, commonly by 10, and frequently by 20 ſeconds. This experiment, with the ſame event, I had the honour of exhibiting before a committee of the Royal Society, for their ſatisfaction, as to the cauſe of the failure of my intended obſervations, September 11, 1762, at the Britiſh Muſeum.

The irregularities in queſtion evidently aroſe from the friction of the loop of the plumb-line againſt the neck of the central pin; a fault, to which moſt of

the sectors, made before mine, have probably been liable. Indeed the fault became more glaring here, by the workman's having made the diameter of the neck of the central pin so large as $\frac{1}{20}$th of an inch; but that the errors cannot be entirely removed by lessening the neck of the pin, I can assert from my own experience, having caused a pin to be made with the neck only $\frac{1}{70}$th of an inch in diameter (and beyond that it cannot well be reduced) by which I still found an irregularity in the suspension of the plumb-line, to the amount of $3''$, a quantity, though seemingly small, yet of great consequence in the nice observations to which this instrument is generally applied, and which it is capable of taking to a prodigious exactness, when the suspension of the plumb-line is accurately provided for. Mr. Bird has contrived one of six foot length, for settling the limits between Pennsilvania and Maryland, in which the plumb-line is adjusted so as to pass over against, and bissect a small point at the centre of the instrument.

I cannot, on this occasion, omit remarking that the late learned Abbé de la Caille's sector, with which he made his principal observations, from some of which I inferred the probability of an annual parallax of Sirius, seems to have had a like fault with my sector, as may be inferred not only from the differences in the observations themselves, but also from the brief account of the suspension of that instrument, contained in a letter with which I have been favoured by M. Delalande from Paris, an extract of which I presented to the Royal Society. Vide Phil. Transf. Vol. LII. Part 2. Page 607.

Let

Let me obſerve alſo, that the 9 foot ſector, made in London by Mr. Graham, with which the gentlemen of the Royal Academy of Sciences at Paris meaſured a degree of the meridian, at the polar circle, and afterwards a degree between Paris and Amiens, had, at the time of their making thoſe obſervations with it, the ſuſpenſion of the plumb-line contrived after the ſame manner as mine (whatever alterations may have ſince been made in it) as appears from M. De Maupertuis's minute and accurate deſcription of the ſaid inſtrument, in his account of the meaſure of the degree of the meridian between Paris and Amiens: for he there ſays, that the part of the central pin, on which the loop of the plumb-line was hung, reſembled the meeting of two oppoſite cones at their points; which is an exact deſcription of the form of the neck of the central pin of my ſector. But, though this capital defect in the ſuſpenſion of the plumb-line of my inſtrument (which I could not correct, at St. Helena, for want of workmen and tools) prevented me from deciding the queſtion concerning the annual parallax of Sirius; yet, as I am conſcious, the want of ſucceſs did not ariſe from any fault of mine, I ſhall endeavour to conſole myſelf for my diſappointment, by the reflexion, that I may, at leaſt, have contributed ſomething to the benefit of aſtronomy, by having diſcovered, by my experiments, the imperfection of the above-mentioned method of ſuſpenſion of the plumb-line in ſectors, which no one ever ſuſpected before, and ſo may be the means of preventing any more inſtruments of this kind being conſtructed in the like

faulty

faulty manner, and consequently any future astronomers being deceived in their observations.

There still remained one object worthy of attention, which I had also proposed to the Royal Society, and received their encouragement to proceed in it. This was the observation of the horary parallaxes of the moon, by the difference of right ascension in time between the moon's enlightened limb, and stars near her parallel of declination: a kind of observation never before made to my knowledge, by any astronomer, in a latitude so near to the equator, as St. Helena; which, by determining the mean horizontal parallax in that latitude, infers also, by a proportion, which will come out sensibly the same upon any probable hypothesis of the figure of the earth, the mean equatorial parallax, which hath never yet been deduced in any manner so nearly direct.

For the purpose of making these observations, I was provided with a polar axis, suitable to the latitude of the place, on which my reflecting telescope was mounted, and a particular additional eye-piece, having fine silver wires stretched in the focus of the nearest eye-glass. The cell containing the wires being moveable round about, by means of a screw, it was easy to cause any star near the moon's parallel of declination to run exactly along one of the wires, which may be called the directing wire, from the centre to the extremity of the field of the telescope. The exact instants of the stars passing three wires placed perpendicular to the former, which may be called the horary wires, representing small portions of horary circles, were noted by the clock

 to

to the exactneſs of $\frac{1}{4}$th of a ſecond of time; as were alſo the inſtants of the moon's enlightened limb paſſing the ſame wires. It is manifeſt that the difference of time, obſerved by the clock, between the ſtar and the moon's limb paſſing the horary wires, reduced to ſidereal time, and from thence into parts of the equator, is the apparent difference of right aſcenſion between the ſtar and the moon's limb paſſing the horary wires. The ſame obſervations repeated after an interval of ſome hours gave the preſent difference of right aſcenſion between the ſtar and the moon's limb; whence the moon's apparent motion in right aſcenſion, or the difference of theſe differences is known; which ſubſtracted from the moon's motion in right aſcenſion, in the given interval of time, owing to her proper motion in her orbit, computed, in the moſt exact manner, from the beſt tables, leaves the remainder for the change of the moon's parallax in right aſcenſion between the two times of obſervation; the ratio of which to the horizontal parallax at that time being alſo computed, the horizontal parallax of the moon is known: and conſequently, by the help of a proportion borrowed from the tables, the mean horizontal parallax of the moon in the latitude where the obſervations are made. The mean horizontal parallax being deduced in this manner from a great many obſervations on different nights, the mean of all the reſults may be taken, as approaching very near to the truth: for the advantage is ſo great from taking a mean of a great number of aſtronomical obſervations, that any degree of exactneſs required, may

be thereby obtained, provided they are not liable to any constant and uniform cause of error: as has been clearly shewn by my late worthy and learned friend Mr. Thomas Simpson, Phil. Transf. Vol. XLIX. Part I. Page 82; and also in his Miscellaneous Tract, Page 64. Therefore I cannot but think, that, from a considerable number of such observations, the mean horizontal parallax, and thence the mean equatorial parallax of the moon might be deduced certainly to a single second, or ever nearer if required.

In hopes of attaining such a degree of exactness, I endeavoured to multiply my observations as much as possible: yet, through the great cloudiness of the island, could not obtain more than three nights complete observations. If these should appear too few to attain the exactness proposed, yet they may contribute, in a good measure, thereto. However, I have since had an opportunity, during my residence at the island of Barbadoes, in the latitude of 13° north, which 3° degrees nearer the line than St. Helena, to repeat these observations to a very great number, from which, I doubt not, the mean equatorial parallax of the moon may be accurately determined.

I shall here desire to remark, that, if the like observations were repeated in different latitudes, they would probably afford the best means yet proposed for ascertaining the true figure of the earth; as they would determine the ratio of the diameters of the parallels of latitude to each other, the horary parallaxes being proportional thereto: and, after all the experiments and observations that have been made

on this ſubject, we ſhall probably, at laſt, be indebted to obſervations of the moon's parallax for the beſt determination of it: for though the earth affords but a ſmall baſe at the moon, yet, by repeating theſe trials, and comparing their reſults, we may hope to attain that degree of exactneſs, which we could never expect from fewer obſervations.

As I look upon the ſpecies of obſervations here ſpoken of to be of very important uſe for the improvement of aſtronomy and geography, and as ſuch deſire to recommend the practice of them, eſpecially to thoſe who may have occaſion to viſit countries of diſtant latitudes; I ſhall briefly mention ſuch further particulars, which the experience I have had, and my attention have ſuggeſted to me, the obſervance of which may conduce to the greater accuracy, as well of the obſervations, as of the conſequences to be deduced from them.

I apprehend the uſe of a polar axis to be very neceſſary for rightly managing the teleſcope, as well for finding what ſtar it is proper to compare the moon with, as for preſerving the poſition of the wires unvaried, after their adjuſtment. A very nice and exact polar axis is not requiſite; but a cheap one, and ſuch an one as may eaſily be provided, will ſuffice. Mine was formed by a braſs ſocket, making an angle with the horizontal top of the ſtand equal to 16° or the latitude of the place, receiving the braſs cylindrical ſupport of the teleſcope, inſtead of the perpendicular ſocket of the common ſtand: and the teleſcope was firmly confined in the ſocket by a pointed ſcrew which paſſed through one ſide of the

the ſocket into ſome of the holes, wich were drilled in the ſupport of the teleſcope.

The polar axis may be ſet near enough to the direction of the meridian, by a magnetic needle, allowing for the variation; or, even by the ſight, provided the walls of the obſervatory be built nearly north and ſouth. This being done, and the directing wire being brought into ſuch a poſition, that the ſtar may run exactly along it from the centre to the extremity of the field of the teleſcope; then if the ſcrews of the rack work be turned, and the ſtar be brought back to the interſection of the wires, it will be found to run exactly along the directing wire again; and this I generally found would be the caſe, even for a very conſiderable ſpace of time, though the ſtar had, in the mean time, advanced a conſiderable way from eaſt to weſt by the diurnal rotation; ſo that it is not always neceſſary to re-adjuſt the wires after each ſett of obſervations, though it may be proper to examine whether they require it or not. Hence it follows, that there can be no danger of diſturbing the poſition of the wires after their adjuſtment, by bringing the ſtar back to the entrance of the teleſcope, in order to obſerve its paſſage acroſs all the horary wires.

Sometimes it ſo happens that a proper ſtar cannot be found that precedes the moon, to compare her with; in ſuch a caſe, the obſerver muſt compare her with a ſtar following her, and adjuſt the wires by making ſome bright point of the moon run along the directing wire, which is a more exact method than by making the directing wire a tangent to the moon's north

north or ſouth limb. Here, indeed, the directing wire cannot repreſent a parallel to the equator, on account of the moon's continual change of declination, but will make a ſmall angle therewith; which may be computed, and the obſervations corrected accordingly. But the correction may be eaſier made, as follows, let *a* expreſs the moon's apparent angular motion about the pole of the world in four minutes of time, being the difference of her proper motion in right aſcenſion, and the change of her parallax in right aſcenſion: *d* her apparent motion and declination in the ſame time, *b* the difference of the apparent declination of the moon, and that of the ſtar, *r* the radius, and *c* the coſine of the moon or ſtar's declination; the correction to be applied to the moon's right aſcenſion, or the difference of right aſcenſion of the moon and ſtar is $b \times \frac{d}{a} \times \frac{r^2}{c^2}$. If the moon is approaching the ſtar's parallel of declination, ſhe will come to the horary wire relatively too late for the ſtar, and her right aſcenſion, deduced immediately from that of the ſtar, will be too great, and muſt be diminiſhed by the correction here mentioned; but, if the moon is receding from the ſtar's parallel of declination, ſhe will come to the horary wire relatively too ſoon for the ſtar, and her right aſcenſion, immediately deduced from that of the ſtar, will be too ſmall, and muſt be increaſed by the above-mentioned correction.

There is another attention, which the nice obſerver will not think too trifling for his notice, namely, to examine whether the wires of his teleſcope are

are placed at exact right angles to each other (which they seldom are) and, if they are not, what the quantity of deviation is, in order to make an allowance for it in the reduction of the observations. This may be tried several ways. I examined the wires of my telescope at Barbados, by a great many observations of the difference of right ascension of stars, which differed considerably from each other in declination, namely Arcturus, and the little star accompanying it, and the virgin's spike, and a small star preceding it; first with the wires in the common position, and next when turned a quarter round, making the middle horary wire serve as a directing wire; for, if the wires do not cut each other at right angles, the difference of right ascension of the stars will come out too much one way, and as much too little the other way, and half the difference will be the correction in this case, whence it may be inferred in all other cases. Or, the angle of the deviation of the wires from a right angle being hence found, the correction of the difference of right ascension of the moon and star, is to the difference of their apparent declination; as the sine of the angle of the deviation of the wires is, to the cosine of the moon or star's declination.

I have determined the deviation of the wires of the telescope, which I used at St. Helena, by comparing them with a right angle, formed by two silver wires on a brass plate, fixed up in a window at the distance of 30 feet from the telescope. The extent of the compasses, with which the intersecting arches were struck, for finding the perpendicular lines

lines on the plate, being no leſs than ſeven inches, thoſe wires may be ſuppoſed to differ inſenſibly from a right angle to each other. The teleſcope being adjuſted for ſeeing them diſtinctly, I brought that wire of the teleſcope, which in celeſtial obſervations repreſented a parallel of declination, to be exactly parallel to one of the wires on the plate, with the ſmalleſt interval poſſible; and, at the ſame time, made the middle perpendicular, or horary wire, to paſs through the interſection of both the wires in the window: when I plainly diſcerned, that the wires of the teleſcope were not exactly perpendicular to each other, the ſuperior angle to the right being manifeſtly acute, and the ſuperior one to the left obtuſe. This I further verified by applying the acute angle to the left hand ſuperior angle of the plate, turning the wires in the teleſcope a quarter round, from right to left, by the ſcrew adapted for this purpoſe, when the ſame difference appeared as before. This proved alſo that the wires on the plate made exact right angles with each other; otherwiſe the acute angle of the wires of the teleſcope could not have appeared to differ equally from both of them. To find the exact difference of the angle made by the wires from a right angle, I had a third wire placed exactly parallel to one of the former on the plate at the diſtance of $\frac{1}{8}$th of an inch; when by applying the angle of the wires of the teleſcope to the right angle on the plate, the deviation of the former from the latter appeared to be equal to half the interval of the parallel wires at the extremity of the field of view; but the ſemi-diameter of the field of the teleſcope at the diſtance of the wires in the window being

being measured $\frac{925}{1000}$th of an inch; whence the angle of deviation of the wires, from a right angle, is 21 minutes. But, by a mean of 11 trials, the quantity of the deviation came out 28′ ½, the extreme results being 21′ and 36′. This is the deviation of the south part of the middle horary wire, from a perpendicularity, to the directing wire towards the east, in the observations at St. Helena; a star, that passed south of the centre of the telescope, coming to the horary wire too soon, and a star that passed north of the centre coming later to the horary wire than it ought to do.

In order to determine whether the two other horary wires were parallel to the middle one, or, if not, what angle they made with it, I compared the transit of 13 stars across the three horary wires, with those of as many more stars differing considerably in declination from the former, all observed at St. Helena; and from the differences of right ascension at the several wires, after making an allowance for the convergence of the meridians, which however is not 2′, I found the south part of the first or eastern wire to deviate from a parallelism with the middle one towards the west by 9′,8, and the western wire to deviate towards the east by 5′,4; hence it appears that the south parts of the eastern, middle, and western horary wire differed from a perpendicularity to the directing wire towards the east, in the observation of St. Helena, by 18′,7, 28′ ½, and 34′, the mean deviation of all the three being 27′ or only 1 ½ different from that of the middle horary wire. This quantity of the mean deviation of the wires is also con-

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firmed by a comparifon of fix differences of right afcenfion of ftais obferved at St. Helena, with the fame obferved, at my defire, fince my return, at the Royal Obfervatory, by the tranfit inftrument, which gives 27′ $\frac{1}{4}$, agreeing exactly with what has been here found in a more certain manner.

Sometimes, in making thefe obfervations, it fo happened, that feveral ftais lay near the moon's parallel of declination; when I obferved all of them, that came within the field of the telefcope; as well to obviate the hazard of miffing to obferve the right ftar again after an interval of feveral hours, as to obtain a greater number of comparifons of the moon's motions in right afcenfion, and fo reduce the unavoidable errors of the obfervations as much as poffible.

It may be proper to remark, that the moft convenient time for making thefe obfervations, is when the moon is ftationary at her greateft declinations; when fhe may be compared with the fame ftar, with a telefcope having a moderate field of view, for feveral hours. The change of the moon's parallax in declination is then alone to be feared; but if the obfervations are made nearly at the fame diftance from the meridian, both on the weftern and eaftern fide, the parallax returning to the fame quantity, will occafion no difficulty. Sometimes, when the moon is not exactly at, but only near, her greateft declination, by obferving her on the proper fide of the meridian, the effect of parallax may be found to be contrary to, and confequently counteract, her change of declination arifing from her proper motion.

Through

Through the great cloudineſs of the ſkies at St. Helena, I could obſerve the moon only one night, namely January 8, 1762, near her limits of declination; on the other nights, I endeavoured to compare her with as many ſtars as came within the field of the teleſcope, truſting to determine afterwards the difference of right aſcenſion of the ſtars, with which ſhe was compared in the former and latter obſervations of the ſame night. This deſign I have ſince completed, by procuring a great many tranſits of theſe ſtars, to be obſerved on the meridian, at the Royal Obſervatory, in the latter end of the year 1762, and beginning of the year 1763; many alſo I made there myſelf. An account of them is given at the end of the other obſervations.

An uſeful remark here offers itſelf to our notice; that the moon's parallax may be very well determined, in a fixed obſervatory, at any period of her declination, by obſerving the difference of right aſcenſion of her limb, and any ſtar near her parallel, at a conſiderable diſtance from the meridian, either to the eaſt or weſt, with the parallactic teleſcope, and alſo on the meridian with the tranſit inſtrument.

But, in purſuing this method, the parallactic teleſcope ought to be nearly of equal goodneſs with the tranſit teleſcope; elſe the moon's diameter might appear greater by ſome ſeconds through one and the other, and conſequently the parallax ſo deduced would not be exact. It is true, that, by a proper method of comparing the obſervations, this ſmall error might be obviated, though the teleſcopes differed ever ſo much in the degree of diſtinctneſs, namely, by taking a mean of the reſults found by

the obſervations in the firſt and laſt half of the moon, or on the eaſtern and weſtern ſides of the meridian; for it is manifeſt the errors would be of contrary tendency in theſe different caſes.

As it may ſerve the more to recommend the practice of theſe obſervations to aſtronomers, I think it proper to mention, that I ſeldom failed of finding ſome proper ſtar or ſtars near the moon, of ſufficient brightneſs, to compare her with, even when there were none ſuch marked down in any catalogue, or any charts: the number of zodiacal ſtars proper for comparing the moon and planets with in a teleſcope, and not inſerted in any printed catalogue, ſeeming much to exceed the number of thoſe marked down.

I have but one more remark to add on this ſubject, that, as it is neceſſary to know nearly the apparent difference of declination of the moon's centre, and the ſtars obſerved, in order to correct the obſervations for the deviation of the wires, and the moon's change of declination, ſo this may moſt readily and conveniently be done by ſome oblique wires fixed in the focus of the eye-glaſs of the teleſcope, as I had in that I uſed at Barbadoes. Not having ſuch a contrivance adapted to the teleſcope I uſed at St. Helena, I always eſtimated by my eye how many minutes each ſtar, and alſo the moon's centre, paſſed north or ſouth of the directing wire, which I had an eaſy method of doing, by comparing their diſtance from the directing wire, with the interval between the directing wire, and one of the two wires ſtretched parallel to it, at the exact diſtance of 10 minutes on each ſide. In this manner the numbers

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Fig. 1.

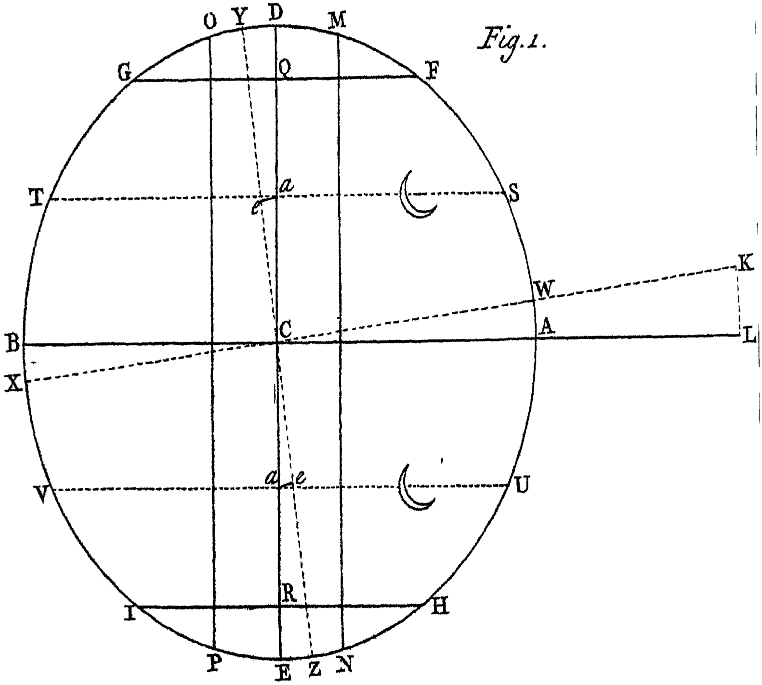
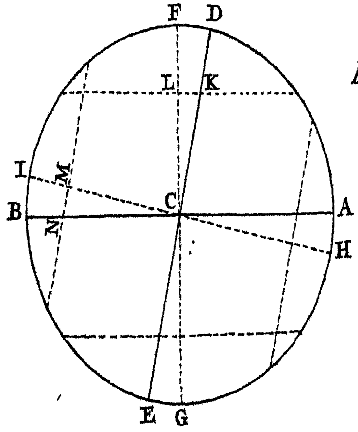


Fig. 2.



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ſet down by the ſides of moſt of the obſervations were deduced, expreſſing the number of minutes which the ſtars paſſed north or ſouth of the moon's centre. They may be depended on to a minute, or two at moſt, which is ſufficient for the reduction of the obſervations.

To make the foregoing account of the method of obſerving the moon's horary parallaxes more clear, let ADBE, Tab. XXI. fig. 1. repreſent the cell containing the ſilver wires ſtretched in the focus of the neareſt eye-glaſs of the teleſcope, of which A B is the directing wire, M N, D E, O P, three wires perpendicular to A B repreſenting portions of horary circles; and, F G, H I, two other wires parallel to the directing wire A B, at the equal diſtances, C Q, C R, on each ſide, equal to 10 minutes. The ſemi-diameter of the field of the teleſcope C A is 14 minutes; A is the eaſtern ſide of the field of view, B the weſtern, D the ſouthern, and E the northern ſide. In order to adjuſt the wire A B parallel to the equator, ſo that M N, D E, O P, may repreſent horary circles, the ſtar, whoſe difference of right aſcenſion from the moon is to be obſerved, is brought by means of the ſcrews to the interſection of the wires A B, D E, at the centre of the teleſcope C, and when the ſtar is near paſſing out of the teleſcope at B, the cell A D B E is turned round, by means of a ſcrew, till the ſtar is again brought upon, and biſſected by the wire A B; this being done, if the ſcrew be turned to make the teleſcope follow the ſtar towards the weſt, and the ſtar be again brought to the interſection of the wires at C, it will be generally found to run exactly along the wire C B, biſſected by it all the way from C to B; which is then a proof that the wire A B is rightly adjuſted; but, if the ſtar

runs

runs not exactly along the wire A B, the position of that wire must be altered a little, till the star runs exactly along it from the centre C, to the extremity of the field of the telescope at B. Then turn the screw to carry back the star from B a little to the east of the first or eastern wire M N, and observe the exact minute, second, and quarter of a second, of the stars passing the three horary wires M N, D E, O P; the telescope remaining unmoved and undisturbed, observe, in like manner, the transit of the moon's enlightened limb across the same wires, whether she pass south of the star, as along the line S T, or north of the same, as along to the line U V; and the observation is completed. The like observation being repeated, after an interval of several hours, we shall have the apparent motion of the moon in right ascension in this time; whence the moon's horary, and thence her horizontal parallax may be computed.

If the moon precedes the star, and the wire A B is adjusted by making some bright point in the moon run along it, and W X is supposed to be the true parallel of declination, it is manifest that the star will pass the horary wire at *a*, to the south of the centre of the telescope sooner, and to the north of the centre of the telescope later, than it passes the true horary circle Y Z at *e*, by the time it takes to describe *a e* parallel to W X. Let C L be the apparent motion of the bright point of the moon, in four minutes of time; draw K L perpendicular to W X produced, and C K is the apparent motion reduced to a parallel of the equator in four minutes, and K L the apparent motion in declination in the same time; and,

 the

the right-angled triangles $a\,e\,C$, K C L being similar, $a\,e$ is to $e\,C$, as K L to C K; but the error of the right aſcenſion anſwering to $a\,e$ is to $a\,e$, as radius to the coſine of the ſtar's declination; and C K, is to the moon's apparent angular motion about the pole, in four minutes, as coſine of moon's apparent declination, to radius. Whence, by compoſition of ratio's, and by equality, the correction of the moon's right aſcenſion is to $e\,C$ the apparent difference of declination of the moon and ſtar, in a compound ratio of the moon's apparent motion, in declination in four minutes, to her apparent motion about the pole in the ſame time, and of the ſquare of the radius, to the product of the coſines of the ſtars and moon's apparent declinations.

Further it appears from the ſcheme, that the moon comes later to C than to the horary circle paſſing through the point of the wire (a) cut by a ſtar between C and D, whoſe parallel of declination ſhe is approaching, and that ſhe comes ſooner to C than to the horary circle paſſing through the point of the wire (a) cut by a ſtar between C and E, whoſe parallel of declination ſhe is receding from, by the time the ſtar takes to deſcribe $a\,e$; and, therefore, the right aſcenſion of the moon deduced immediately from that of the ſtar muſt be too great in the firſt caſe, and too little in the ſecond caſe, by the ſpace $a\,e$ meaſured upon the ſtar's parallel of declination.

Laſtly, to explain the manner of examining the deviation of the wires, from a perpendicular to each other, by obſervations of the ſtars; let A B, fig. 2. repreſent the directing, and E D the middle horary wire, deviating from C F, ſuppoſed perpendicular

to

to A B, by the ſmall angle D C F. Let any ſtar be made to run along the wire A B, from A to B, any other ſtar following it will paſs the wire D E, at K ſooner, than the horary circle F G at L, by the time of its deſcribing the ſmall ſpace L K, and conſequently the difference of right aſcenſion will appear too little; now let the wires be turned a quarter round, that the wires A B, D E, may change places, D coming into the place of A, and E into that of B, which is done by making the firſt ſtar run along the wire D E, from C to E. Now the wire A B, deviating from C I perpendicular to D E, by the angle B C I, the ſecond ſtar will paſs the wire B C at N, later than the horary circle I C at M, by the time it takes to deſcribe the ſpace M N = L K; and conſequently the difference of right aſcenſion of the two ſtars will appear as much too great, as it before appeared too little, when the wires were adjuſted in their uſual poſition: and half the difference will be the correction in this caſe, to be added to the firſt, or ſubtracted from the ſecond difference of right aſcenſion; whence the correction may be eaſily inferred for all other obſervations.

Celestial Observations.

Eclipse of the Moon, May 18, 1761.

Appt Time. H	′	″	
7	48	38	Penumbra plainly entered upon the Moon's disc.
7	56	23	Beginning of the Eclipse.
7	19	16	Shadow begins to touch Kepler.
7	37	8	Shadow bissects Manilius.
7	40	8	Shadow begins to touch Julius Cæsar.
10	39	23	Emersion out of total darkness.
11	46	52	End of the Eclipse.

Immersions and Emersions of Jupiter's Satellites, observed at the Observatory on the Alarum Hill.

1761 Day of the Month.	Appt Time H	′	″	
☽ July 20	17	44	31	Imm. 1 Sat. The Moon near Jupiter.
☿ July 22	12	12	29	Imm. 1 Sat.
☿ Aug. 5	14	49	50	3 Sat. almost immerg'd. Then clouds.
	14	50	30	3 Sat. certainly immerg'd.
	16	0	40	Imm. 1 Sat.
♃ Aug. 27	11	13	56	Emers. 4 Sat.

Immerſions and Emerſions of Jupiter's Satellites, obſerved in James's Valley.

1761. Day of the Month.			Apparent Time. H	′	″	
♄	Oct.	10	12	48	38	Em. 2 Sat.
♃	Oct.	15	13	36	10 :	Em. 1 Sat.
♀	Oct.	16	10	3	40	Em. 3 Sat.
			10	3	45	Ditto by Mr. Maſon (who arrived here to-day from the Cape of Good Hope) with a two foot reflecting teleſcope.
☽	Nov.	9	8	19	54	Em. 1 Sat. Inſtantaneous both to myſelf and Mr. Maſon, exactly at the ſame ſecond, in different houſes. Air very clear, and Satellite increaſed its light very faſt.
☿	Nov.	11	12	40	19	Em. 2 Sat. by Mr. Maſon. I miſſed the inſtant of Emerſion by moving the ſtand of my teleſcope. Air very clear.
☽	Nov.	23	12	9	52	Em. 1 Sat.
♄	Nov.	28	7	39	18	Imm. 3 Sat. Air a little hazy.
			7	39	13	Ditto by Mr. Maſon.
			10	8	17	3 Sat. had not em. Then clouds.
			10	10	13	Ditto plainly out, though not near arriv'd to its full luſtre.
☉	Nov.	29	7	9	48	2 Sat. had emerg'd. Its light was ſo weak, it probably had not been out above 15 ſeconds.
☉	Dec.	6	9	43	36	Em. 2 Sat.
			9	43	33	Ditto by Mr. Maſon.
1762.						
☉	Jan.	10	7	37	42	Imm. 3 Sat.

N. B. This mark (:) affixed to any obſervation ſignifies that it is a little uncertain.

Obſervations

Observations of the difference of right ascension, by time, between the moon's enlightened limb and stars, taken by the help of the parallactic wires adapted to the reflecting telescope; designed for determining the horary parallaxes of the moon. They may also serve to deduce the longitude of the place.

♃ Sept. 3. Compared the moon's western and preceding limb, with α Libræ in right ascension, at the Observatory on the Alarum Hill. The times are by the clock, which is 12 seconds too slow for sidereal time, and keep the rate of going of sidereal time, exactly. The star passed south of the moon's centre.

		1st hor. w.		2d hor. w.		3d hor. w.		☽'s Limb at middle wire. Apparent Time.		
	H	′	″	′	″	′	″	H	′	″
α Libræ	17	42.	9	42	23	42	37			
☽'s W. Limb	17	43	45	43	59½	44	13½	6	52	49
α Libræ	18	2	10	2	24	2	38			
☽'s W. Limb	18	4	14	4	28½	4	42½	7	13	15
α Libræ	18	16	3	16	17	16	31			
☽'s W. Limb	18	18	27½	18	42	18	56	7	27	26
α Libræ	18	23	6	23	20	23	34			
☽'s W. Limb	18	25	41−	25	55	26	9+	7	34	38

Thursday September, 24 I removed the clock down to James's Valley, and keeping the same length of the pendulum as before, fixed it up strongly against the wall of a house, in an upper room, whence I could make my observations through openings made in the roof of the house. I fixed the equal altitude instrument, for regulating the clock, against a strong post, let deep into the ground, in a little room eight foot square, built for this purpose, in a convenient open place, at a little distance from

 the

the houſe where the clock was. When I obſerved the ſun's equal altitudes, I firſt adjuſted the inſtrument; then I went to the room where the clock was, and ſet my watch, having a ſecond hand, exactly with it; then I returned to the equal altitude inſtrument, and obſerved the paſſage of the ſun's limb acroſs the horizontal wires of the inſtrument, according to the time ſhewn by the watch; and, immediately after the obſervation, went again to the clock, and compared the watch with it, noting how much it had got or loſt, whence the obſervations were eaſily reduced to the time of the clock.

			1 Wire		2 Wire		3 Wire		Apparent Time		
		H	′	″	′	″	′	″	H	′	″
♃ Oct. 8	☽'s W. L.	11	34	2	34	17	34	31½	10	45	14
	ε Capricorni	11	Clouds		36	30½	Clouds		Star 10′ N. of M.		
♄ Oct. 10	☽'s W. L.	14	19	57	20	12–	20	25	13	23	7
	ψ Aquarii	14	24	57	25	11	25	24½			

October 8th and 10th the clock got 7″,3 upon 24 hours in one revolution of the fixed ſtars.

The foregoing obſervations were all made with the teleſcope placed upon the common ſtand, without the polar axis. The obſervations of December 4th, that follow, were made with the teleſcope fixed upon a new and heavier ſtand, which was rendered more ſteady, by two broad feet reſting upon ſeveral of the boards at once. The ſocket for receiving the teleſcope was cut obliquely in the ſtand, ſo that it had partly the effect of a polar axis.

October 28th, I took down the clock, packed it up, and ſent it on board a veſſel going to the Cape of Good Hope, to return again ſoon, committing it to the care of Mr. Jeremiah Dixon, who had obſerved the tranſit of Venus over the ſun at the Cape. He took his paſſage on board the ſaid veſſel, in order to ſet the clock up at the Cape of Good Hope, and examine the difference of its going between that place and St. Helena, for determining the proportion of the force of gravity at thoſe two places.

The ſame day Mr. Maſon fixed his clock up, for my uſe, againſt a large maſſy poſt, let deep into the ground, near the equal altitude inſtrument, at the little Obſervatory. This clock was made by Mr. John Ellicott, F. R. S.

I ſtill

I ſtill continued, for ſome time, to make my obſervations in the upper room, as before. For this purpoſe I fixed up a little clock there, which may be called a journeyman, or ſecondary clock, having a pendulum ſwinging ſeconds, which after being well adjuſted, would keep time very regularly for ſeveral hours. It had only a minute and ſecond hands, and ſtruck every minute exactly as the ſecond hand came to ſixty, which, was very convenient for the counting of ſeconds; more eſpecially in the obſervations made with the parallactic teleſcope, it being improper, on account of the inſtability of the floor, to get up from one's ſeat, or to alter the poſition of the body conſiderably even to catch the ſecond, till thoſe obſervations were completed. I reduced the times to that of the obſervatory clock, by means of my watch, with the ſecond hand. The little clock, as well as the larger clock, which I ſent with Mr. Dixon to the Cape of Good Hope, was made by Mr. John Shelton.

♀ December 4th, I compared the moon's weſtern limb with the three * of Aquarius, with reſpect to right aſcenſion, and obſerved her occult to the ſouthern one. The time is ſet down according to the little clock, and the difference between that and the obſervatory clock is ſet down by the ſide; the latter loſt 59 ſeconds upon 24 hours, in one revolution of the fixed ſtars, and the little clock kept very nearly the rate of ſidereal time. In theſe obſervations the directing wire was adjuſted by the ſtars.

1 Wire

	1 Wire	2 Wire	3 Wire	Lit. clock slow. than obs. clock	☽'s L. at mid. Wire by observatory clock	Appt Time
	H ′ ″	′ ″	′ ″	′ ″	H ′ ″	H ′ ″
☽'s W. Limb	23 21 0	21 13½	21 27	8. 4	23 29 17½	7 19 5
3d ψ Aquarii	23 23 32	23 46–	23 59—			
☽'s W. Limb	23 38 31	38 45	38 59	8. 4	23 46 49	7 36 34
3d ψ Aquar.	23 40 46	40 59½	41 13			
☽'s W. Limb	0 47 17	47 31–	47 44	8. 0	0 55 31	8 45 6
3d ψ Aquar.	0 48 18	46 32	48 45½			
☽'s W. Limb	0 58 0	58 15–	58 28	8. 0	1. 6. 15	8 55 48
3d ψ Aquar.	0 58 50	59 3½	59 17			
2d ψ Aquar.	1 4 10	4 24	4 37			
☽'s W. Limb	1 4 30	Clouds	4 58+	8. 0	1 12 44	9 2 17
☽'s W. Limb	1 17 33	17 47	18 1	7 59	1 25 46	9 15 17
3d ψ Aquar.	Clouds	18 13½	Clouds			
2d ψ Aquar.	1 25 7½	25 21½	25 35			
☽'s W. Limb	1 25 54	26 8	26 22	7 59	1 34 7	9 23 37
1st ψ Aquar.	1 31 27	31 40	31 54			
2d ψ Aquar.	1 33 33	33 47	34 0			
☽'s W. Limb	1 34 30	34 44	34 58	7 58	1 42 42	9 32 10
1st ψ Aquar.	1 45 31½	45 45½	45 59½			
2d ψ Aquar.	1 47 37	47 52	48 5+			
☽'s W. Limb	1 48 53½	49 7½	49 21	7 58	1 57 5	9 46 31
1st ψ Aquar.	1 58 57	59 11	59 24½			
☽'s W. Limb	2 2 37½	2 52	3 5½	7 57	2 10 49	10 0 13
1st ψ Aquar.	2 9 16	9 30	9 43			
☽'s W. Limb	2 13 11	13 25	13 39	7 57	2 21 22	11 10 45
1st ψ Aquar.	2 20 6+	20 20	20 33½			
2d ψ Aquar.	Clouds	22 26	Clouds			
☽'s W. Limb	2 24 17	24 31½	24 45	7 56	2 32 27	10 21 48

At

At 23 H. 49 M. 48 S. by little clock, or 23 H. 57 M. 51 S. by obſervatory clock, which is 7 H. 47 M. 34 S. apparent time, the 3d ✶ of Aquarius vaniſhed inſtantaneouſly, clouds coming over the moon at the ſame time. Therefore it remains a little dubious, whether this was the very inſtant of the ſtar's occultation by the moon, or whether it was obſcured by the clouds, though I rather ſuppoſe the former from the manner of its vaniſhing, and alſo becauſe when the clouds cleared away preſently the ſtar was gone.

December 4^{th}, by equal altitudes, the ſun paſſed the meridian at 16 H. 9 M. 11,3 S. and December 5^{th} at 16 H. 12 M. 33,4 S. by the obſervatory clock, whence the obſerved times are eaſily reduced to apparent time, as above.

Finding the above obſervations of December 4^{th} (though they may be depended on to half a ſecond of time) to be ſtill incommoded by a ſmall trembling of the teleſcope, owing to its reſting on a floor; I determined, for the future, to make theſe obſervations, at the little obſervatory, on the ground, which I cauſed to be altered, to make it more convenient for this purpoſe. Here I conſtantly made uſe of the polar axis, which I found to afford conſiderable advantages with reſpect to the facility and exactneſs of making the obſervations.

♀ January 8^{th} 1762. Compared the moon's weſtern limb, with ſeveral ſtars, with reſpect to right aſcenſion, at the little obſervatory. The four ſtars, with which the moon was compared, are diſtinguiſhed by letters, according to the order of their right aſcenſion.

	1st Wire	2d Wire	3d Wire	Stars North or South of ☽'s centre	☽'s Limb at Mid. Wire Apparent Time
	H ′ ″	′ ″	′ ″	′	H ′ ″
a	2 48 30—	48 45	49 0	13½ N.	
d	Clouds	55 53+	55 48+	4½ N.	
e	5 56 10+	56 25+	Clouds	15½ N.	
	2 57 3	57 18	Clouds		8 41 58
a	3 16 32	17	17 22+	14½ N.	
b	3 19 12—	19 27	19 42+	22 N.	
c	3 22 38	22 53+	Clouds	13½ N.	
☽	3 26 16	26 32	26 47½		9 11 8
a	4 22 38—	22 53—	23 8—	12 N.	
b	4 24 57	25 13—	25 27½	19 N.	
c	4 Clouds	28 39	28 54	11¼ N.	
e	4 30 18	Clouds	30 48—	12½ N.	
☽	4 33 55	34 11½	34 27		10 18 38
a	4 52 44—	52 59	53 14		
b	4 Clouds	55 19	55 34	b 7′ N. of a	
☽	5 4 54:	5 9½:	Clouds		10 49 31½
	5 15 9	Clouds	15 39½		
	5 20 4½	20 20	20 35½		11 4 40
*	5 30 52	31 7½	31 22½		
a	5 33 15	33 30	33 45	20 circ. of S. *	
a	5 57 39	57 54+	58 9+	6½ N.	
b	5 59 59+	0 14½		13½ N.	
c	6 3 25½	3 40½	3 55½	6½ N.	
e	6 5 20	5 35	5 50	7 $\frac{7}{10}$ N.	
☽	6 11 44	12 0—	12 15		11 56 12
*	6 18 18½	18 34	18 49	20 cir. S. of *	
a	6 20 43	20 58	21 13	13½ S. of ditto	
b	6 23 2	Clouds	23 32½	21½ S. of ditto	
c	6 26 28+	26 43½	26 59	19½ S. of ditto	
e	6 28 23	28 38+	28 53—		
	7 55 56	56 11½	56 26+	10 S.	
	8 5 56	6 12	6 27		13 50 9

♄ January

January 9. Compared ☽'s weſtern limb with three ſtars. Adjuſted the directing wire by a bright point in the moon.

	1ſt Wire	2d Wire	3d Wire	Stars N. or S. of ☽'s centre	☽'s Limb at Mid. Wire Apparent Time
	H ′ ″	′ ″	′ ″	′	H ′ ″
☽ L.	3 59 42	59 58	0 13½		9 41 8
1 *	4	17 cir.		10 cir. S.	
2 *	4	19 42		1 N.	
3 *	4 23 10	23 26—	23 40½	11 ½ S.	

January 9. from equal altitudes, the ſun paſſed the meridian at 18 H. 17 M. 29 S. and January 10, at 18 H. 20 M. 50,6 S. per clock, which loſes 59 S. upon the rate of ſidereal time in one revolution of the fixed ſtars: therefore the ſun may be computed to have paſt the meridian, January 8, at 18 H. 14 M. 7 S.

☿	February 3				
a	4 49 31	49 45½	50 0	The minute not noted, but from following obſervations muſt have been 50 M. to middle wire.	
b	4 50	17	32		
☽	4 53 23½	53 59	53 54—		7 44 28
b	5 0 6½	0 21	0 36—		
☽	5 3 42	3 57	4 12		7 54 44

♀	February 5				
☽	5 0 0½	0 16	0 31½		7 42 52
*	5		10 13	6 N.	
☽	5 44 12½	44 28½	44 44—		8 26 56
*	5 52 40	52 55	53 10	5 N.	
☽	5 57 7—	57 23—	57 38		8 39 48
*	6 5 12+	5 27½	5 43		
☽	6 22 24	22 39	22 55		9 5 0
*	6	30 3	30.18		

	1ſt Wire	2d Wire	3d Wire	Stars N. or S. of ☽'s centre	☽'s Limb at Mid. Wire
	H ′ ″	′ ″	′ ″	′	Apparent Time H ′ ″
⊙	February 7				
☽	6 36 5	36 20	36 35		9 10 29
	6	51 ½ cir.			
g	6 58 48		59 17½	7 N.	
i	6 59 49+	0 . 4		12 N.	
l	7 1 24			12 S.	
☽	7 24 4−	24 19	24 34		9 58 20
	7	39 cir.		13 S.	
l	7 48 2	48 17	48 32	3 1/7 S.	
q	8 2 7	2 22	2 36+	5 ¾ S.	
☽	9 7 10	7 25	7 40		11 41 8
a	9		16 1+	10 S.	
b	9		18 20		
c			18 32		
h	9 26 3	26 17½	26 32	7 ½ S.	
k	9		27 46		
l	9	28 38−	28 52½	8 8/10 N.	
m	9	32 15			
n	9 32 18	32 32	32 46½		
o	9 36 42	36 57+	37 11+	6 S.	
p	9 38 29	38 43½	38 58+	6 N.	
q	9 42 27+	42 42	42 56½	5 ¾ N.	
☽	10 9 38½	9 53½	10 9−		12 43 26
c	10 18 51	19 6	19 20+	14 N.	
f	10	26 32		0	
k	10	28 21			
l	10	29 27		22 N.	
q	10 43 16	43 31	43 46	18 2/3 N.	
☽	10 51 34½	51 49½	52 4+		13 25 15
a	10 57 8−	57 23		13 ¾ N.	
f	11	7 17+	7 32	9 ½ N.	

	1 Wire	2 Wire	3 Wire	Stars N. or S. of ☽'s centre	☽'s Limb at Mid. Wire Apparent Time
	H ′ ″	′ ″	′ ″	′	H ′ ″
☽	12 22 10	22 25+	22 40+		14 55 36
d	12 29 16	29 31—	29 45+	12 $\frac{1}{10}$ S.	
☽	12 36 17+	36 32$\frac{1}{2}$	36 48—		15 9 44
d	12 42 54$\frac{1}{2}$	43 9	43 24—	9 $\frac{1}{10}$ S.	
e	12	44 9—	44 23$\frac{1}{2}$	12 S.	
☽	12 53 10	53 26—	53 40+		15 26 31
d	12 59 13—	59 27	59 41$\frac{1}{2}$		
e	13 0 12	0 27—	0 41		
☽	13 7 27	7 42	7 57		15 40 45
d	13 12 58$\frac{1}{2}$	13 13	13 28		
☽	13 21 32	21 48—	22 3		15 54 48
e	13	27 48+		3 S.	

February 9

3 59 42	59 58	0 13$\frac{1}{2}$		6 26 26
4	17 cir.		11 S.	
4	19 42		1 N.	
4 23 10	23 26—	23 40$\frac{1}{2}$	11 S.	

The obſervations from February 3^{d} were made with my own clock, with which Mr. Dixon returned from the Cape of Good Hope December 30th, 1761, after examining the going of it there. He found it to get there 36,6 ſeconds upon ſidereal time in one revolution of the fixed ſtars, or 29,3 ſeconds *per* day more than it got at St. Helena with the ſame length of pendulum: but I propoſe to give a more particular account of theſe, and ſome other experiments then made by Mr. Dixon at the Cape, ſome other opportunity.

From equal altitudes, the ſun paſſed the meridian, January 30th, at 20^{h} 51′ 8″ $\frac{1}{4}$; February 5th, at 21^{h} 16′ 5″,6; and February 7th, at 21^{h} 24′ 17″,7. Hence the clock appears to

have got at the rate of 6″,3 upon ſidereal time in 24 hours. By the ſetting of four ſtars behind the hill, obſerved with the teleſcope of the equal altitude inſtrument, January 29th, and again February 7th and 8th (after the manner deſcribed by Mr. Maſon in his account of the going of Mr. Ellicott's clock determined by him and myſelf in this manner, Phil. Tranſ. Vol. LII. Part II. Page 534.) the clock appeared to get 6″,25 upon ſidereal time in one revolution of the ſtars, which agrees exactly with the former determination by the ſun's equal altitudes. In like manner, I always found the going of the clock, determined by theſe two different methods, would come out as nearly the ſame as the equal altitudes of the ſun could be depended upon, that is to ſay, to a ſecond, even from the obſervations of two ſucceſſive days.

I muſt not paſs by this occaſion, without taking notice of ſome remarks, which Mr. Short paſſes on my method of examining the going of the clock, by obſerving ſtars ſetting behind a hill, with the teleſcope of the equal altitude inſtrument; (vide Mr. Short's account of Mr. Maſon's paper concerning the going of Mr. Ellicott's clock at St. Helena. Phil. Tranſ. Vol. LII. Part II. Page 540). Mr. Short repreſents Mr. Maſon, as ſaying in his paper, that I propoſed making uſe of the equal altitude inſtrument to determine the regularity of the motion of Mr. Ellicott's clock, by obſerving the vaniſhing of the ſtars out of the field of the teleſcope, an expreſſion not contained in Mr. Maſon's paper, who is only ſpeaking of our obſerving ſtars ſetting behind a hill, at the diſtance of a quarter of a mile, in the ſame part of the field of the equal altitude inſtrument. Had we proceeded in the method ſuppoſed in the remarks, no doubt the obſervations would have been liable to conſiderable inaccuracy: but as we uſed the teleſcope of the equal altitude inſtrument, only to aſſiſt the ſight in obſerving the ſtars ſetting behind the hill, we were liable to no other error than what might ariſe from the ſmall alterations of the inſtrument, ariſing from the changes of heat and cold, moiſture and drineſs, ſeen from the diſtance of the top of the hill, which will eaſily be allowed to be quite inſenſible. And, indeed, how otherwiſe could the obſervations, contained in Mr. Maſon's paper, agree ſo well together as they do? A circumſtance alone ſufficient to create a ſuſpicion of the objection being ill grounded. The reaſon of Mr. Maſon and myſelf always obſerving the ſtars to vaniſh behind the hill, in the ſame part of the field of the teleſcope (that

is,

is, very near its centre) was, in order to keep the object glass at the same height; though this being less than an inch in diameter, and consequently subtending less than 13″ from the top of the hill, there could not have been a second of time difference, whether the stars had been observed to vanish behind the hill, either in the upper or lower part of the field of view.

Mr. Short also remarks, that no inference can be formed with respect to the different forces of gravity, in different latitudes, from experiments made with clocks, because the same clock, set up on different sides of the same room, will be found to differ considerably from itself. I readily allow that, if clocks are fixed up in a slight manner, or against common wainscots, the experiments made with them cannot be depended upon. Nevertheless it does not appear, but that when they are fixed in a firmer manner, they may be depended upon near enough to be of a considerable use in physical enquiries: which I have reason to think from the many experiments I have tried with the Royal Society's clock, made by Mr. John Shelton, which I propose to give a particular account of at some other oportunity.

Observations of the Sun's setting in the Sea.

At the Observatory at the Alarum-House, which, by careful mensuration, I found to be elevated 1983 feet above the level of the sea. Therefore the height of the eye is 1988 feet.

1761	Appt Time H	′	″	
June 14	5	39	$5\frac{1}{2}$	☉'s upper limb set in the sea.
June 16	5.	39.	1 :	☉'s upper limb set. A little cloudy.
July 18	5	39	34	☉'s L. L. set in the sea, certain to 2 or 3 S.
	5	42	8	☉'s U. L. set in the sea, certain to $\frac{1}{2}$ S.
July 30	5	44	21 :	☉'s U. L. set in the sea. A little cloudy.

The

The like Obſervations made in James's-Valley, near the Sea-ſide.

1761	Appt Time H ′ ″		
Nov. 16	6 23 42	☉'s L. L. ſet	Height of eye above the ſea 16 feet.
	6 25 22	☉'s centre ſet	
	6 26 21	☉'s U. L. ſet	
Dec. 15	6 29 24	☉'s L. L. ſet	Height of eye above the ſea 15 feet.
	6 32 10	☉'s U. L. ſet	
Dec. 16	6 32 11½	☉'s U. L. ſet, thro' a ſmall apperture in a cloud. The height of eye above the ſea 15 feet.	
Dec. 17	6 29 24	☉'s L. L. ſet	Height of eye above the ſea 15 feet.
	6 30 52½	☉'s centre ſet	
	6 32 20½	☉'s U. L. ſet	

☿ Auguſt 5th
Appt Time

H ′ ″	
5. 6. 7	A ſtar 6th magn. in Piſces, at centre of equal altitude inſtrument.
5. 6. 32	Jupiter's centre at the horizontal wire.
5 7 8	Jupiter's centre at the vertical wire.

The teleſcope remained fixed during theſe obſervations.

Again.

17 31 30	The ſtar at the vertical wire.
17 31 46	The ſtar at the horizontal wire.
17 32 11	Jupiter's centre at the horizontal wire.
17 32 37	Jupiter's centre at the vertical wire.

Here alſo the teleſcope remained fixed during the obſervations.

☿ Auguſt 19th. Found the little ſtar, which is 14″ N. of Scorpii, to precede it one ſecond of time, by my parallactic

wires, with my watch, which makes four beats in a ſecond of time. If any thing the difference was ſomething more than a ſecond of time: the little ſtar may therefore be ſuppoſed to precede β Scorpii 17″ in right aſcenſion. By obſervations made with the 10 foot ſector on ſeveral nights, while β Scorpii and the little ſtar were paſſing the field of the teleſcope, I found the little ſtar to be exactly 14″ N. of β Scorpii in declination. For June 24 the difference was 12″,5. July 21, 13″,6. July 22, 14″,7. July 23, 15″,3: Auguſt 14, 12″,9. Auguſt 17, 14″,8.

The bright ſtar in the foot of the Centaur, marked α in the catalogues, when viewed through a teleſcope, becomes divided into two ſtars, one of which is about the ſecond, and the other about the fourth magnitude. They were both obſerved by the Abbé de la Caille. I found their diſtance, by the divided object glaſs micrometer fitted to the reflecting teleſcope, to be 15″ or 16″. But it is, in a manner, impoſſible to meaſure the diſtance of two ſtars very accurately with this micrometer, for being ſimilar lucid objects, when they are brought very near each other, their light will be confounded together before they exactly coincide.

The larger Magellanic cloud, viewed through a teleſcope, exhibits a few ſtars which then appear ſeparated to a conſiderable diſtance from each other. Their being ſo few in number, and ſo thinly ſcattered, is the reaſon of this phænomenon appearing ſo very faint.

The leſs Magellanic cloud, viewed through a teleſcope, exhibits a very remarkable lucid nebula, with ſome tolerable bright ſtars ſurrounding it. The nebula appears nearly circular, about 3′ in diameter.

Tranſits

Tranſits of ſtars, with which the moon was compared, February 7, 1762, at St. Helena, taken upon the meridian, at the Royal Obſervatory, in the beginning of the year 1763, for aſcertaining the exact difference of their right aſcenſion. Vide page 363.

		1 Wire	2 Wire	3 Wire	4 Wire	5 Wire	
		′ ″	′ ″	H ′ ″	′ ″	′ ″	
January 16	*a*		52 52	8 53 31	54 21		
	c	54 41	55 20+	8 55 59+			
	d		57 17	57 56	58 35	59 14	ξ of Cancer
	e	57 38	58 16½	58 56	59 35	0 13½	
	h	2 42::	3 21::	9 4 1:			
	f					4 44	
	l		5 40+	6 20	6 59	7 38½	
	o			9 13 39:			
	q	19 5	19 44+	9 20 23½	21 3	21 14½	λ of the Lion

Note, the clock gets five ſeconds per day upon ſidereal time.

		1 Wire	2 Wire	3 Wire	4 Wire	5 Wire
March 6	*c*		52 39	8 53 18	53 57	54 36
	h			9 1 19	1 58½	
	i				2 42	
	l				4 18	4 57
	m				7 34	8 14
	q			17 43—	18 22	19 1

Clock loſes one ſecond and an half per day.

		1 Wire	2 Wire	3 Wire	4 Wire	5 Wire
March 7	*a*		50 10	8 50 49+	51 29	
	c	51 59	51 37+	53 17	53 56+	
	d		54 34½	55 13½	55 52½	56 31
	e		55 33½	56 14	56 52½	57 31
	h			9 1 17½		
	l		2 58	3 37	4 17	4 55½
	q		17 2	17 41	18 21	19 0
March 8	*a*			8	51 27	
	c	51 57	52 37	53 15		
	d	53 54	54 32½	55 12	55 51	56 29½
	e	54 54	55 33	56 12	56 51	57 29½
	i	0 41		9 2 0	2 39	3 18
	l				4 15+	4 53
	q	16 21	17 0	17 39½	18 19	18 57½

		1 Wire	2 Wire	3 Wire	4 Wire	5 Wire
		′ ″	′ ″	H ′ ″	′ ″	′ ″
March 9	a	49 28	50 7	8 50 45½	51 25	
	c	51 55+	52 34	53 13—		
	d	53 52½	54 31	55 10	55 49	56 27
	e	54 52½	55 31	56 10	56 49	57 28
	f			9 0 41		
	h			1 14+	1 55:	
	l		2 54½	3 34	4 13½	4 52
	n			7 30		
	o			10 54:	11 34:	
	q	16 20—	16 59	17 38	18 17+	18 56
March 12	a			8	51 52	52 31
	c	52 23	53 2	8 53 41	54 20½	
	d		54 59	55 38	56 16½	
	e		55 59	56 39	57 17	57 55
	g			9	2 5	
	l					5 19
	q		17 26—	9 18 6	18 45	19 24
March 13	a		50 33—	8 51 12	51 51+	52 30½
	c	52 22	53 0.∴	53 40		
	d	54 19	54 57+	55 36½	56 15½	
	e	55 19	55 57	56 37½	57 15+	57 54
March 15	a			8 51 11	51 50	52 29
	c	52 21	53 0+	53 39		
	d		55 57	56 36		
	e		56 57	57 36½		
	q	16 46	17 25	9 18 4+	18 44	19 22+

Clock loses $\frac{5}{6}$ second per day.

April 6	a			8 50 54	51 33	
	c	52 5	52 43	53 22		
	d	54 1	54 40	55 19	55 58	
	e	55 1	55 40	56 19	56 58	
	g		59 25	9 1 7		
	i	0 48½::	1 27	2 7		
	q	16 28	17 7+	9 17 47	18 26	19 5½

Clock loses $\frac{3}{4}$ second per day.

N. B. By a mean of ſeveral tranſits of ſtars obſerved about this time, as well as by the foregoing obſervations, it appears that the intervals of the four firſt wires are exactly equal, and that the interval of the two laſt wires is ſmaller than the others by $\frac{1}{25}$th part, which anſwers to $\frac{4}{10}$th of a ſecond of time in the tranſits of the above ſtars.

 By

By ſeveral tranſits over the meridian, obſerved at the Royal Obſervatory, at the latter end of the year 1762 and beginning of 1763, the firſt or the preceding ſtar of the three ♆'s of Aquarius (with all which I compared the moon, December 4, 1761, at St. Helena) preceded the ſecond or ſubſequent one 2′.7″,11 of time in right aſcenſion, and the third or laſt 3′.9″,70 and the ſecond preceded the third 1.′2″,59 all according to the time of a clock regulated to agree with the diurnal revolution of the ſtars.

LVIII. *An Account of an extraordinary Diſeaſe among the* Indians, *in the Iſlands of* Nantucket *and* Marthu's Vineyard, *in* New England. *In a Letter from* Andrew Oliver, *Eſq; Secretary of his Majeſty's Province of* Maſſachuſett's Bay, *to* Iſrael Mauduit, *Eſq; F. R. S.*

Boſton, 26 Oct. 1764.

SIR,

Read Dec. 20, 1764.

CONSIDERING your connexions, both as a member of the Royal Society, and of the Society for propagating the goſpel among the Indians, I tranſmit you an account of an uncommon ſickneſs, which prevailed the laſt year at the iſlands of Nantucket and Martha's Vineyard, which lie about ſix or ſeven leagues from each other, and the latter about four or five leagues diſtant from the Indian plantation at Maſhpee on the Continent, where it did not make its appearance at all. As I had my account from the Engliſh miniſter, and from the phyſician at Nantucket, and from the ſociety's miſ-

ſionary

ſionary at the Vineyard, of each of whom I made the moſt ſcrupulous inquiry, you may depend on the truth of it.

About the beginning of Auguſt, 1763, when the ſickneſs began at Nantucket, the whole number of Indians belonging to that iſland was 358: of theſe, 258 had the diſtemper betwixt that time and the 20th of February following, 36 only of whom recovered: of the 100, who eſcaped the diſtemper, 34 were converſant with the ſick, eight dwelt ſeparate, 18 were at ſea, and 40 lived in Engliſh families. The phyſician informs me, that the blood and juices appeared to be highly putrid, and that the diſeaſe was attended with a violent inflammatory fever, which carried them off in about five days. The ſeaſon was uncommonly moiſt and cold, and the diſtemper began originally among them; but having once made its appearance ſeems to have been propagated by contagion; although ſome eſcaped it, who were expoſed to the infection.

The diſtemper made its appearance at Martha's Vineyard the beginning of December, 1763. It went through every family, into which it came, not one eſcaping it: fifty-two Indians had it, 39 of whom died; thoſe, who recovered, were chiefly of the younger ſort.

The appearance of the diſtemper was much the ſame in both theſe iſlands; it carried them off in each, in five or ſix days. What is ſtill more remarkable than even the great mortality of the diſtemper is, that not one Engliſh perſon had it in either of the iſlands, although the Engliſh greatly exceed in numbers; and that ſome perſons in one

family, who were of a mixt breed, half Dutch and half Indian, and one in another family, half Indian and half Negro, had the diſtemper, and all recovered; and that no perſon at all died of it, but ſuch as were intirely of Indian blood. From hence it was called the Indian Sickneſs.

There had been a great ſcarcity of corn among the Indians the preceding winter: this, together with the cold moiſt ſeaſon, have been aſſigned by ſome as the cauſes of the diſtemper among them. Theſe circumſtances, it is true, may have diſpoſed them to a morbid habit, but do not account for its peculiarity to the Indians: the Engliſh breathed the ſame air, and ſuffered in ſome meaſure in the ſcarcity, with the Indians; they yet eſcaped the ſickneſs. I do not ſee therefore, but that the Sudor Anglicus, which heretofore affected the Engliſh only, and this late Indian ſickneſs, muſt be claſſed together among the Arcana of Providence.

I am,

SIR,

Your moſt obedient

Humble ſervant,

Andrew Oliver.

LIX. *Astronomical Observations made at the Island of* Barbados; *at* Willoughby Fort; *and at the Observatory on* Constitution Hill, *both adjoining to* Bridge Town. *By* Nevil Maskelyne, *A. M. Fellow of* Trinity College, Cambridge, *and F. R. S.*

[Read December 20, 1764.]

N. B. All the observations of the first satellite of Jupiter were made with a two foot reflector made by Mr. Bird, having an aperture of 3 $\frac{8}{10}$ inches in diameter, except where they are marked as made with my 18 inch reflector made by Mr. Short, having an aperture of four inches and an half in diameter. And the eclipses of the other satellites were all observed with the 18 inch telescope, except a few marked as made with Mr. Bird's two foot reflector. The latitude of Willoughby Fort is 13.° 5′ N. and that of the Observatory 13.° 5.′ 15″ N. This agrees with the latitude I observed when our ship came to anchor at our first arrival in Carlisle Bay, which, carefully calculated, and reduced to St. Michael's Church, gives 13.° 5.′ 30″ for its latitude. The Observatory is about 200 yards due east of the church, and Willoughby Fort is about 1600 feet distant from the same, bearing S. W. So that the Observatory is about 15″ of longitude, or 1″ of time to the east of Willoughby Fort. The latitudes of Willoughby Fort and the Observatory, set down above, result from altitudes of the sun taken with a bason of quick-silver by the brass Hadley's sextant made by Mr. Bird, being calculated from them, and the apparent time known from equal altitudes of the sun taken with the equal altitude instrument.

At

1763 Nov.	App^{t} Time. H ′ ″	At Willoughby Fort.
☉ 13	15 5 38	Immersion of 1st Satellite of Jupiter.
♀ 18	5 44 41	☉ set totally, or his upper limb set in the sea, height of my eye above the sea about 10 foot.
☉ 20	13 21 15:	Imm. 2 Sat. of Jupiter. Air hazy and moon-shine.
	16 57 20:	Imm. 1 Sat. of Jupiter through thin clouds.
♂ 22	11 26 6	Imm. 1 Sat. of Jupiter.
♃ 24	13 52 29	Emersion of ψ Cancri four magn. from the moon's dark limb, dubious to 5″, by taking my eye off the telescope for 10″.
Dec.		
♃ 1.	7 46 0::	Imm. 1 Sat. of Jupit. 18 inch telescope, magnifies 170. Air hazy; yet the satellite was separate from the body of Jupiter.
♂ 6	12 10 45	Emers. 3 Sat. of Jupit. 18 inch magnifies, 170.
	17 21 4::	Emers. 1 Sat. of Jupit. low towards the horizon. 18 inch telescope.
♃ 8	10 24 55	Emers. 2 Sat. of Jupit. } 18 inch mag. 170 { Sat. at a sensible.
	11 46 35	Emers. 1 Sat. of Jupit. } 18 inch mag. 170 { Sat. at a very sensible distance from Jupiter's body.
♃ 15	12 59 46	Emers. 2 Sat. of Jupiter.
	13 37 9	Emers. 1 Sat. of Jupiter.
♄ 17	8 5 46:	Emers. 1 Sat. of Jupiter. Air hazy.
☿ 21	14 12 0	A small telescopic star preceded Jupiter's centre 1.′ 34″ in right ascension, and was 7″ N. of the same in declination.
♃ 22	15 28 35	Emers. 1 Sat. of Jupiter.
	15 35 35::	Emers. 2 Sat. of Jupiter. 2 foot telescope. Almost touched the 1 Sat. at the emersion.
♄ 24	9 56 20	Emers. 1 Sat. of Jupiter.

1764 Jan.		At the Observatory.
♄ 7	13 39 43:	Emers. of 1 Sat. of Jupiter. Air a little hazy, but Jupiter sufficiently bright.
☽ 9	8 7 20	Emers. 1 Sat. of Jupiter.
	8 31 18	A telescopic star (inserted in Senex's map of the Zodiac) in the line of the moon's horns produced, and ½ a minute distant from moon's south horn.
	10 2 8	Emers. of 2 Sat. of Jupiter.
☽ 16	9 59 38	Emers. of 1 Sat. of Jupiter.
	12 37 59	Emers. of 2 Sat. of Jupiter.
☿ 18	9 47 46	Imm. of 3 Sat. of Jupiter.
	12 0 13	Emers. of 3 Sat. of Jupiter.

App^{t}

1764 Jan.	Appt Time H ′ ″		
♄ 21	10 59 28 11 23 18	ϰ Leonis (which was occulted by the ☽ this night in Europe) precedes ☽'s subsequent limb in right ascension by the parallactic wires of my 18 inch telescope.	2. 8. 30 2. 17. 46
☽ 23	11 52 36	Emers. of 1 Sat. of Jupiter. Emerg'd instantaneously.	
☿ 25	6 20 51	Emers. of 1 Sat. of Jupiter. A good observation; though only 40′ after sun set, sufficiently dark.	
Feb.			
☿ 8	9 43 24	A star 6.7 magn. in Aries, in line of ☽'s horns, produced 1′ S. of ☽'s south horn. Certain to 10″.	
	10 9 19	Emers. of 1 Sat. of Jupiter.	
♀ 17	8 24 39	Occultation of *l* Leonis 6 magn. by ☽'s bright limb, certain to 5″. Full moon this day.	
☽ 20	11 50 32 12 7 6	The Virgin's spike precedes ☽'s subsequent limb in right ascension by parallactic wires.	1° 51′ 11″ 1 57 33
	15 7 37	Ditto by transit instrument upon the meridian	2 55 28
		N. B. The Virgin's spike was occulted by the ☽ this night in Europe.	
♃ 23	7 54 8	Internal contact of one of Jupiter's satellites, with his east limb at its entrance upon his body.	
	7 54 44	Sat. disappears. Suppose it has been visible 36″ upon his body.	
	8 5 3	Emers. of 3 Sat. of Jupiter.	
	13 48 46	Antares follows ☽'s subsequent limb in right ascension by parallactic wires of 18 inch telescope.	8.′ 10.″
♀ 24	8 30 42	Emers. of 1 Sat. of Jupiter.	
March			
♂ 6	7 5 12	Emers. of 2 Sat. of Jupiter. Air very clear.	
♃ 8	7 2 43	Emers. of *c* Pleiadum from the ☽'s bright limb, certain to 10″.	
♄ 10	7 21 46 9 24 49	β Tauri, 2 magn. precedes ☽'s preceding limb in right ascension by parallactic wires.	1.° 55′ 28″ 2 41 26
☽ 12	9 49 22½	Occultation of *υ* Gemini 5 magn. by ☽'s dark limb.	
	11 9 15	Emers. of ditto from ☽'s bright limb, certain to 10″.	
♂ 13	7 56 7	Occultation of 2 *υ* Cancri by ☽'s dark limb.	
	9 13 29	Occultation of 3 *υ* Cancri by ☽'s dark limb.	
♀ 16	11 17 18	Occultation of *σ* Leonis 4.5 magn. by ☽'s dark limb.	
♄ 17 Eclipse of ☽	6 41 28	Beginning of the eclipse of the ☽.	
	7 26 11	Shadow at middle of Mount Ætna.	
	7 36 29	Shadow of the middle of Mount Hercules.	
	9 26 50	End of the eclipse.	

N. B. I observed the beginning and end with an opera glass of Dollond's construction.

Appt

1764	Appt Time H	′	″				
April							
♀ 6	8	28	11	Emerſ. of 3 Sat. of Jupiter.			
	8	33	30	3 Sat. arrived to its greateſt brightneſs.			
♀ 13	13	2	32	Imm. of β Virginis into ☽'s dark limb.			
	13	48	48	Emerſ. of ditto from ☽'s bright limb, certain to 10″.			
☉ 15	7	19	17	The Virgin's ſpike precedes ☽'s preceding limb in right aſcenſion by parallactic wires		49.′	43.″
				N. B. It was occulted this night in Europe.			
May							
♃ 10	7	7	47	Imm. of σ Leonis 4.5 magn. into ☽'s dark limb.			
June							
☿ 6	7	41	4	Imm. of χ Leonis 4.5 magn. into ☽'s dark limb.			
	8	53	33	Emerſ. of ditto from ☽'s bright limb. Certain to 5″.			
♃ 14	11	13	14	τ Sagittarii 4 magn. precedes ☽'s ſubſequent limb in right aſcenſion by parallactic wires.	1.°	8.′	46″
July							
☽ 9	9	51	56	Antares follows ☽'s preceding limb in right aſcenſion by parallactic wires.	3.°	8.′	48″
	13	22	27		1	32	c
♃ 12	8	39	11	τ Sagittarii 4 magn. prec. ☽'s prec. limb in right aſcenſion by parallactic wires.	6	21	33
	10	31	23		7	9	19
Aug.							
☉ 5	9	1	28	Occultation of π Scorpii 3 magn. by ☽'s dark limb.			
♀ 10	7	41	43	Occultation of χ Capricorni 6 magn. by ☽'s dark limb, but which was ſo near to being full, that the ſtar ſeemed to vaniſh in the illuminated arch terminating light and darkneſs.			

Beſides the above obſervations, I have taken a great many of the difference of right aſcenſion between the ☽'s enlightened limb and proper ſtars (which I have not yet reduced) by means of parallactic wires in the focus of my 18 inch reflecting teleſcope; from which, after making the requiſite calculations, I make no doubt of being able to deduce the moon's horizontal parallax in that latitude, and thence, by proportion, the equatorial parallax of the moon with great exactneſs, which has never been done yet in ſo direct a manner.

LX. *Remarks*

LX. *Farther Remarks upon* M. l'Abbé Barthelemy's *Memoir on the* Phœnician *Letters, containing his Reflections on certain* Phœnician *Monuments, and the Alphabets resulting from them. In a Letter to the Rev.* Thomas Birch, *D. D. Secretary to the Royal Society, from the Rev.* John Swinton, *B. D. F. R. S. Member of the Academy* degli Apatisti *at* Florence, *and of the* Etruscan *Academy of* Cortona *in* Tuscany.

Reverend Sir,

Read Dec. 13, 1764.

M. L'Abbé Barthelemy's memoir on the Phœnician letters has again, with very large additions, been just (1) communicated to the learned world. Some at least of those additions have been made, as there is exceeding good reason to believe, if not very lately, several years after the memoir itself was read. This, as M. l'Abbé is said to be the first antiquary in France, and must undoubtedly have a great influence over the members of that illustrious body which he has so long adorned, cannot well fail of being considered by many people as a confirmation of the suspicion for some time

(1) *Mémoires de Litterature, tirés des Regiſtres &c.* Tom. XXX. p. 405–426. A Paris, 1764.

entertained

entertained in ſeveral parts of Europe, and hinted at by me in (2) a former paper. It will therefore enable us to account for the late publication of a piece, which ſeems to have been cried up by M. l'Abbé's admirers as one (3) of the moſt valuable literary productions of the preſent age. What degree of attention to this performance from the lovers of antiquity is really due, I ſhall not at preſent take upon me to decide. My ſentiments of it, however, if not yet ſufficiently known, from the following ſhort additional remarks, ſubmitted with the utmoſt deference to the ſuperior judgment of the Royal Society, will very clearly appear.

I.

M. l'Abbé ſtill aſſerts, that צורא, TZORA, or TZVRA, in the firſt line of the Malteſe-Phœnician inſcription, [TAB. XXII.] denotes the city of Tyre. To which I ſhall only beg leave to reply, that this aſſertion is utterly repugnant to the teſtimony of the Tyrian coins; which conſtantly exhibit the word צר, TZOR, or TZVR, as the name of that city. This is a fact expreſly allowed by (4) M. l'Abbe himſelf, though he produces it in ſupport of the notion here advanced; with which it muſt, even at firſt ſight, be conſidered as altogether incompatible.

To the firſt letter of the next word he ſtill likewiſe attributes the power of *He*, (5) and conſequently affirms that word to be הנדר, HOC VOTVM, THIS VOW.

(2) *Philoſoph. Tranſ.* Vol. L. Par. II. p. 799. Lond. 1759.

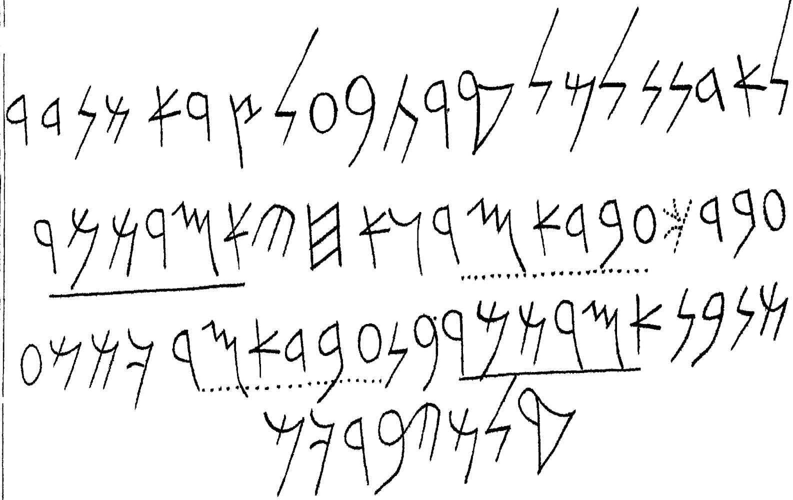
(3) *Journ. des Sçavans*, Decembre 1760. p. 348. M. de Guign. *De l'Orig. des Chin.* p. 60. A Paris, 1760.

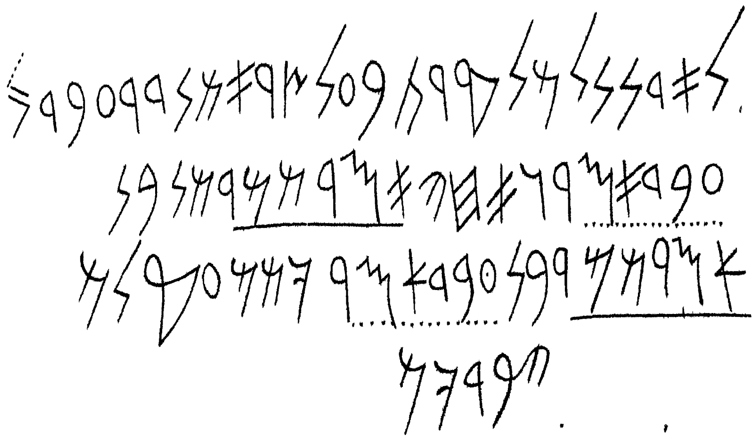
(4) *Mémoir. de Litter. &c.* ubi ſup. p. 409.

(5) Ibid. p. 410, 413.

But

Two Transcripts of the Maltese-Phœnician *Inscription.*





J. Mynde sc.

But as the abſurdity of this notion was clearly demonſtrated in my former remarks, to which no reply has yet been made, I ſhall take no farther notice of it here.

We are alſo told by this learned (6) author, that the firſt word of the ſecond line was either עברן or עברנא, with the *Nun* and *Aleph* ſo cloſely connected as to form a kind of monogram; the fainteſt traces of which are, however, ſcarcely, if at all, to be ſeen.

That the word cut originally in the ſtone was עברו, FECERVNT, in my former remarks (7), I have rendered ſufficiently clear. And that the two elements *Nun* and *Aleph* ſhould be ſo confined as to occupy a ſpace barely ſufficient for one of them, will not be readily admitted by any perſon moderately acquainted with the manner of writing obſerved in the later Phœnician inſcriptions. But to wave all other conſiderations relative to the point in view, that the verb here is expreſſed in the third perſon plural, the laſt word of the inſcription, יברכם, BENEDICAT IIS, ſeems evidently to prove. 'Tis worthy obſervation, that M. l'Abbé repreſents Count Caylus's copy of the inſcription as much ſuperior in point of accuracy to Father Gori's; and yet, in determining, or rather attempting to determine, the form of the laſt letter of עברו, he apparently prefers the latter to the former. For he adduces Father Gori's copy in ſupport, or rather confirmation, of the other. But ſuch conduct as this, in M. l'Abbé's ſituation, is to me no great matter of ſurprize. It is plainly intended to ſerve a favourite hypotheſis, which cannot be eaſily maintained.

(6) *Mémoir. de Litter. &c.* ubi ſup. p. 410.
(7) See above, p. 126, 127.

 With

With regard to the proper name עבדאסר, ABDASAR, or ABDASARVS, I can by no means believe it to be the ſame with ABDASTARTVS. That thoſe two words had not the ſame origin, ſeems to me, at firſt ſight, ſelf-evident. This is likewiſe confirmed (8) by a writer of great erudition. But the account I have (9) already given of the Phœnician name עבדאסר, ABDASAR, ſupported by the beſt authorities, will, I flatter myſelf, ſet this matter in the cleareſt light.

(10) I formerly obſerved, that ASERIMOR, ASERIMAR, or ASERIM-HAMMAR, was probably compoſed of ASERIM, or ASERYM, ΑΣΕΡΥΜΟΣ, the name of one of the kings of Tyre, according to Menander Epheſius, and מר, MAR, or rather המר, HAMMAR, IPSE DOMINVS. But M. l'Abbé, (11) in the piece before me, takes it to be perfectly equivalent to the word ΑΣΕΡΥΜΟΣ itſelf; the Greeks ſeeming to him to have terminated in ΟΣ the Phœnician proper names ending in ΟΡ, as the other natives of Greece did ſeveral words uſed by the Lacedæmonians of the ſame termination. To which I ſhall beg leave to reply, that the Greek dialect of the Lacedæmonians was widely different from the Phœnician tongue; and conſequently that all arguments drawn from their ſuppoſed agreement, or affinity, muſt be fallacious and inconcluſive. Nor will the compoſition of the name אסרם־מר, or אסרם־המר, ASERIM-HAMMAR, viz. אשרה־מר, ASHERAH-MAR, LV-

(8) Matth. Hiller. *Onomaſt. Sacr.* p. 590. Tubingæ, 1706.
(9) See above, p. 127, 128.
(10) See above, p. 129.
(11) *Mém. de Litter.* ubi ſup. p. 410, 411.

CVS

CVS DOMINVS, which he exhibits to our view, afford a proper degree of ſatisfaction to any rational perſon ingaged in ſuch philological inquiries. Farther, it is obvious to every ſmatterer in the Greek language, that in the words ΕΙΡΩΜΟΣ, ΑΒΔΑΣΤΑΡΤΟΣ, ΑΣΕΡΥΜΟΣ, ΒΑΛΕΑΖΑΡΟΣ, ΒΑΔΕΖΩΡΟΣ, &c. HIROMVS, ABDASTARTVS, ASERYMVS, BALEAZARVS, BADEZORVS, &c. handed down to us by Joſephus (12), from Menander Epheſius, ΟΣ is no part of the Phœnician names, but only a Greek termination ſuperadded to them. The word ASERIMAR therefore, or ASERIMOR, would have become, when adopted by a Greek, ΑΣΕΡΙΜΑΡΟΣ, or ΑΣΕΡΥΜΩΡΟΣ, not ΑΣΕΡΥΜΟΣ, as M. l'Abbé has been pleaſed to aſſert. So the Tyrian, or Phœnician, proper name חירם, HIRAM, or HIROM, as it occurs in Scripture, (1 King. ix. 12.) is rendered by the Septuagint and Joſephus, after Menander Epheſius, ΕΙΡΩΜΟΣ, HIROMVS. But the moſt ſtriking inſtances, or rather thoſe directly in point, are ΒΑΛΕΑΖΑΡΟΣ, ΒΑΔΕΖΩΡΟΣ, or BALEAZAR, BADEZOR, when ſtripped of their Greek termination; with which ASERIMAR, or ASERIMOR, does moſt perfectly agree. This amounts to the ſtrongeſt preſumption, that M. l'Abbé's notion of the compoſition of that name is deſtitute of every ſupport. Hence we may fairly conclude, that the account by me formerly given of the conſtituent parts of this word was ſtrictly agreeable to truth; and conſequently that the fourth element was *Mem*, and not *He*, as I then inconteſtably proved.

(12) Menand. Epheſ. apud Joſeph. *Cont. Apion.* Lib. I. p. 1043.

With

With respect to the four first letters of the third line, "they form a difficulty, says (13) this celebrated "writer, embarassing enough. The two last give "the word בן, BEN, FILIUS, SON; but this word "here ought to be read in the plural number. Was "not the plural number sometimes pointed out "amongst the Phœnicians by the addition of an *He* "and a *Nun*, in the same manner as the Chaldees "had an *Aleph* added to the beginning and the end "of this word also in the singular? Or rather did "not the two letters, *He*, *Nun*, forming the pronoun הן, IS, ILLE, give us to understand, that "Abdassar and Asseremor were brothers only by "adoption? I dare not decide in this matter, and "shall content myself with observing, that the difficulty regards only the language of the Phœnicians, of which we are totally ignorant, and by "no means the powers of the letters, which have "been sufficiently established in this memoir."

Here 'tis obvious, at first sight, that M. l'Abbé is not only embarassed, but seems actually to sink under the weight of the difficulty he has himself proposed; as of this, notwithstanding his uncommon sagacity and penetration, he has not been able to supply his readers with an adequate solution. For 1. The word הנבן cannot be of the plural number, as he supposes, or rather positively asserts; both the correspondent Greek and the tenor of the inscription, unless I am greatly deceived, being utterly repugnant to such a supposition. 2. The Chaldee term he mentions being of the singular number, and absolutely dissimilar to the pretended Phœnician word,

(13) *Mém. de Litter.* ubi sup. p. 411.

הנבן;

הנבן; the adduction of it seems altogether impertinent, on the present occasion. 3. The pronoun הן is equivalent to the Latin EARVM, not IS, ILLE, as M. l'Abbé is pleased to affirm; and being a suffix, or affix, adheres to the end, not the beginning, of a word. But if it should be taken for a separate pronoun, it answers to the Latin ILLÆ, not IS, ILLE, as it is rendered by M. l'Abbé. That Abdasar and Aserimar therefore were brothers only by adoption, is a chimerical notion, void of even the least shadow of rational proof. 4. He in effect declares the difficulty to be insoluble, when he owns himself incapable of deciding in this matter. 5. He makes the same declaration, when he asserts the difficulty to regard only the Phœnician language; which he affirms to be utterly unknown, though both he and M. de Guignes have in express terms affirmed it to be almost intirely the same with the Syriac, and he has himself attempted to explain several inscriptions in it. Nor will it in the least avail him to refer the difficulty to the Phœnician tongue, or rather our ignorance of that tongue. For he undertook the interpretation of the whole inscription he has here so minutely considered, in order to deduce a Phœnician alphabet from it; and unless he has, in some measure at least, effected this, how can he take upon him to ascertain the powers of the letters of which his alphabet is composed? I would therefore, in my turn, beg leave to ask him the following question. Is it not more ingenuous, more liberal, and more worthy M. l'Abbé Barthelemy's (14) exalted merit, to retract an errour

(14) *Journ. des Sçavans*, Aout 1760. p. 277. (1).

 than

than to perſiſt in it? To give up a point than to attempt the defence of it, when he ſeems even to look upon it himſelf as altogether indefenſible?

The laſt word but one of the inſcription M. l'Abbé repreſents by the Hebrew characters כהמעקלם, taking the ſecond letter for *He*. He has likewiſe given us a pretty good account of מעקלם, the latter part of it; but has only juſt mentioned the particle כה, formed of the two firſt letters. And in this he has, perhaps, acted prudently enough. For none of the ſignifications of that particle, at leaſt none that I can find, will accord with the ſenſe of that part of the inſcription in which it occurs; whereas if we ſuppoſe the ſecond element to be *Mem*, as I am fully perſuaded it is, every difficulty will immediately vaniſh. This I have clearly demonſtrated in my former remarks. I muſt beg leave farther to obſerve, as pertinent to the preſent occaſion, that as a variety of winds will give a variety of directions to a ſhip's motion, any navigation effected by different winds may be termed crooked, any voyage performed by their aſſiſtance oblique. So that we cannot infer from either of M. l'Abbé's tranſlations of the word in queſtion, that the two Tyrians mentioned in our inſcription were thrown upon the iſland of Malta by a tempeſt, as this learned author has been pleaſed to aſſert. The latter of thoſe tranſlations was owing, as he informs us, to "the favour of certain ſuppo"ſitions, which, for brevity's ſake, he has ſup"preſſed."

From what has been ſaid it appears, that the character denominated *He* by M. l'Abbé, and by me *Mem*, is an object of ſome importance, with regard to the explication of this inſcription. If it be taken for

for the former element, two or three parts of the monument are ſo involved that they are ſcarce, if at all, intelligible; if for the latter, the ſenſe runs throughout unembaraſſed, conſiſtent, and clear. Nor does this character differ more from that allowed to ſtand for *Mem*, by M. l'Abbé, than do ſeveral of the acknowledged ſomewhat different forms of *Mem* from one another. A draught of it, however, ſeems not to have been yet publiſhed, by M. l'Abbé, that can be abſolutely depended upon. For in the copy he firſt communicated to the learned world, repreſented by him as a tranſcript perfectly agreeing with the original, this character approaches a little nearer to his figure of *Mem* than it does in the firſt plate of of the memoir before me, and conſequently the latter ſeems a little more favourable to his hypotheſis. Whether this minute alteration, which is too inconſiderable to affect the point in queſtion, ought to be attributed to the inattention or incapacity of the engraver, or to ſome other cauſe, I ſhall not take upon me to decide. How that matter really ſtands is beſt known to M. l'Abbé.

"But the power of the letter *He*", ſays M. l'Abbé, "is fixed by other examples which I ſhall ſoon "produce." Now that a character repreſenting *He* does not occur in the monument under conſideration, has already, I flatter myſelf, been rendered ſufficiently clear; whether or no it is to be met with on any of the coins produced in the paper before me, by M. l'Abbé, comes therefore next to be conſidered here.

II.

My explications of the firſt, ſecond, and ſixth medals in M. l'Abbé's plate of coins may be ſeen in a ſmall Latin diſſertation, put to the preſs at Oxford, in 1753. That of the ſixth, however, which is a medal of Laodicea, muſt be owned to be incomplete; the four laſt letters of the inſcription having been defaced on my coin, by the injuries of time. Nor am I intirely ſatisfied with M. l'Abbé's interpretation of the latter part of this inſcription, as it ſeems very forced and unnatural, and even contrary to the faith of hiſtory. Nay, it ſeems not perfectly to pleaſe M. l'Abbé himſelf, as he has not abſolutely ſettled the power of one of the letters of which it is compoſed; but contents himſelf with (15) obſerving, "that this ſlight difficulty will "hereafter be removed by other monuments." His interpretation of the inſcription exhibited by the coin of Sidon, which he makes coeval with the reign of Antiochus IV. is likewiſe liable to exception, as will appear to every one moderately verſed in this branch of literature, who examines it with proper attention. The other two pieces of Sidon preſent nothing very remarkable to our view. One of them has nevertheleſs handed down to us the very character aſſerted by me to repreſent *Trade*, but taken by M. l'Abbé Barthelemy for *Thau*, immediately preceding certain numeral characters, which have been fully explained in one of my former papers, on the reverſes of ſeveral Sidonian coins.

(15) *Mém. de Litter.* ubi ſup. p. 417.

Now

Now the letter *He* does not appear on any of these medals, and consequently nothing can be inferred from any of them in favour of the form of that element contended for by M. l'Abbé. On one of those coins, however, said by this learned antiquary to have been struck at Marathus, but which in reality ought not to be attributed to that city, now in my possession, the very same character occurs, with the power of *Mem*, that M. l'Abbé exhibits on two of the medals of Menæ as occupying the place of *He*. My explication of this coin, which I then took to belong to Marathus, was printed here, in 1753. But I afterwards observed, that the Phœnician inscription on this medal consisted of four letters, מרתב, the last of which was *Beth*; and that on all the similar medals, or draughts of them, which I had seen, four characters likewise appeared, the fourth of which was either *Beth* or manifestly a part of that element, not *Ajin* or *Ain*, as M. l'Abbé, without any manner of foundation, seems to imagine. Hence I concluded, that these pieces could never have been struck at מרת, MARATH, or MARATHUS, and therefore scrupled not a moment to explode my former opinion. To this I was farther excited by the numeral characters in the exergues of two of them, at present a part of my small collection of Phœnician coins; which, if I am not greatly mistaken, clearly point out the years of Rome 748 and 749. But about that time Marathus was either in ruins or intirely razed, and the territory appertaining to it occupied by the Aradians, according to Strabo (16).

(16) Strab. *Geograph.* Lib. XVI. p. 753. Lutetiæ Parisiorum, 1620.

I therefore cancelled that part of my ſmall work in which the interpretation of the inſcription, preſerved by one of theſe Phœnician medals, was contained. The cancelled part is, however, ſtill in my hands

Of the Sicilian medals in M. l'Abbé's plate four are to be atrributed to one city, and two to two others. Of the latter M. l'Abbé aſſigns one to Imachara, and the other to Carthage; with what truth, I ſhall not take upon me at preſent to decide. But that a perſon ſo juſtly celebrated for his knowledge of antient medals, particularly Punic and Phœnician medals, as is M. l'Abbé, ſhould firſt aſcribe the former to I know not what Caſtra Cæcilia, or Caſtra Julia, and afterwards to Panormus, now Palermo, is to me, I muſt confeſs, real matter of ſurprize. For the Punic name on theſe coins is evidently מחנות, MAHHANOTH, MEHNOTH, or, as *Hheth* is ſometimes diveſted of even the force of an aſpirate (17), MENOTH; which apparently anſwers to the Greek MHNAI, and the Latin MENÆ, the name of a city in Sicily, called Menéo by Cluverius (18), ſeveral of whoſe medals adorn the cabinets of the curious at this day. Nay, one of the Punic coins of Menæ publiſhed by M. l'Abbé, though without any explication of the Punic inſcription, an dconſequently without ſufficient proof of the point in view, has been expreſly attributed to Menæ, or Menéo, by Goltzius (19). An accurate deſcription of a medal of Menæ, together with a complete interpretation of the Punic inſcription it exhibits, may

(17) Bochart. *Phal.* Lib. I. c. i.
(18) Phil. Cluver. *Sicil. Antiq.* Lib. II. c. ix. p. 339.
(19) Hubertus Goltzius, in *Num. Sicil.* Tab. XII. num. 5, 6.

be

be feen in the Latin differtation (20) above referred to, which it would be fuperfluous to touch upon here.

With regard to the Punic element taken for *He* by M. l'Abbé, on two Siculo-Punic coins, he feems to give up in one part of his memoir the form of it fo warmly by him contended for in another. For he exprefly allows, that this character on one of the Siculo-Punic medals exhibiting it may reprefent *Mem*, as well as *He*. From whence we may infer, that the correfpondent letter on the other, as the word to which this character belongs is on both medals the fame, may likewife, with no fmall appearance of truth, be taken for *Mem*; and confequently that, according to M. l'Abbé, the letter *He* may be fuppofed never in reality to have exifted on either of thofe Siculo-Punic coins.

'Tis obfervable, that on one of the coins of Menæ, in M. l'Abbé's plate, the words קרת חדשת, VRBS NOVA, feem to appear; and that the firft element of this infcription, if the draught of it here may be depended upon, is the *Koph* of nearly the Chaldee form. I have, however, a Punic medal in my collection, (fee TAB. XXI.) with the old Phœnician *Koppa* on the reverfe, and the names of two Sicilian cities, in Punic characters, never hitherto publifhed. A galeated head, with a fprig of laurel before it, on one fide, prefents itfelf to our view; and the triquetra, or fymbol of Sicily, with a human face in the middle of it, appears on the reverfe,

(20) Swint. *De Num. quibufd. Samaritan. & Phœnic. &c. Differt.* Oxon. 1753.

attended

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attended by the words MAGEL, CAMIC, the Punic names of two Sicilian towns. The latter of theſe was the CAMICVS (21) of Diodorus Siculus and Herodotus, and the former the MACELLA, or MAGELLA, from whence the MAGELLINI of Pliny (22), of Dio (23) and Polybius (24). The letters of the inſcription are drawn from the left hand to the right, contrary to the uſual Punic manner of writing. But ſuch accidental miſtakes of the moneyers as this are ſometimes viſible on antient coins. This valuable medal, which formerly had a place in Lord Winchelſea's noble collection, corrects the famous Bochart, with reſpect to the origin of the name CAMICVS; and has preſerved an unuſual Punic form of *Lamed*, agreeing with a Samaritan (25) one of the ſame element, as well as a figure of *Ghimel*, that not ſeldom occurs on the Siculo-Punic coins. Some may, however, conſider this medal as of Greek extraction; the third letter ſo nearly reſembling *Lambda*, and the antient Sicilian Greeks, on certain occaſions, having uſed the Phœnician *Koppa* upon their coins, as appears from the medals of Syracuſe. But as the *Mem* is plainly Punic, or Phœnician, and both the Greek terminations wanting here, I can by no means prevail upon myſelf to ſubſcribe to ſuch an opinion.

(21) Diod. Sic. Lib. IV. & XXIII. Herodot. Lib. VII.
(22) Plin. *Nat. Hiſt.* Lib. III. c. viii.
(23) Dio in Excerpt. apud Porphyrogennet. p. 637.
(24) Polyb. Lib. I. p. 24.
(25) *Numiſm. Antiq. &c.* à Thom. Pemb. & Mont. Gomer. Com. *Collect.* P. ii. T. 85. num. 5.

As

As the Carthaginians therefore used, on certain occasions, the old Phœnician *Koppa*, or one of the earliest forms of *Koph*; we may from thence conclude, that the character (26) on a Punic coin by me long ago explained, so similar to that form, must undoubtedly be taken for the same element, as I then most clearly evinced. Nor has either M. l'Abbé, or any other French writer, hitherto overthrown this notion; though it has been called in question, if not denied (27), by M. Pellerin. Nay, it has been, in a manner, adopted some months since, in the *Journal des Sçavans*, (28) and even by M. l'Abbé himself, in the (29) celebrated memoir that is the object of my attention here.

It has been just observed, that the *Lamed* on the Sicilian coin last described is of an unusual form. Give me leave to add, that another of my Punic medals struck in Sicily has preserved a form of that letter, somewhat more similar to the correspondent character in the alphabet deduced by M. l'Abbé from several Siculo-Punic coins. This medal has on one side the head of Jupiter, and on the reverse two ears of corn, attended by the Punic inscription ALICA, or HALICAH, the ΑΛΙΚΥΑΙ, or HALICYÆ, of (30) Diodorus Siculus, situated between Entella and Lilybæum, according to (31) Cellarius. The

(26) *De Num. quibusd. Samaritan. & Phœnic. &c. Differt.* p. 86, 87. Oxon. 1750.

(27) *Récueil de Medailles de Peuples & de Villes, &c.* Tom. III. p. 141, 142, à Paris, 1763.

(28) *Journ. des Sçavans*, Aout 1763. p. 280.

(29) *Mém. de Litter. ubi sup.* p. 414.

(30) Diod. Sic. Lib. XIV. c. 25.

(31) Christ. Cellar. *Geograph. Ant.* Lib. II. c. 12.

coin

coin has not hitherto been publiſhed, I believe, by any author whatſoever.

Amongſt my Siculo-Punic coins there is likewiſe one with the letters *Hbeth* and *Beth*, on the reverſe. Thoſe elements undoubtedly form part of the word חבלא, HIBLA, or HYBLA, the Punic name of a town, or rather three towns, of Sicily, according to Bochart. That a mint was erected in one of them, at leaſt, from an antient medal, with the words ΥΒΛΑΣ ΜΕΓΑΛΑΣ impreſſed upon it, may be certainly inferred. The Greek cities of Sicily not ſeldom exhibited on their money only part of their names, as we learn from (32) ſeveral of the Greek Sicilian coins; and that the towns there under the dominion of Carthage did the ſame, we have all the reaſon in the world to believe.

Another medal alſo appears in my little cabinet with the character repreſenting *Hbeth* only, on the reverſe. That character may be taken for the initial letter of the Punic proper name חבלא, HIBLA, or HYBLA; and may indicate this piece to have been ſtruck in the city ſo called, as well as the former. Inſtances of ſuch initial letters as this pretty frequently occur, on ſeveral of the (33) Greek Sicilian coins.

Two of my ſmall Carthaginian medals have preſerved the letter *Ghimel*, on their reverſes, of the uſual Punic or Phœnician form. This ſeems to be

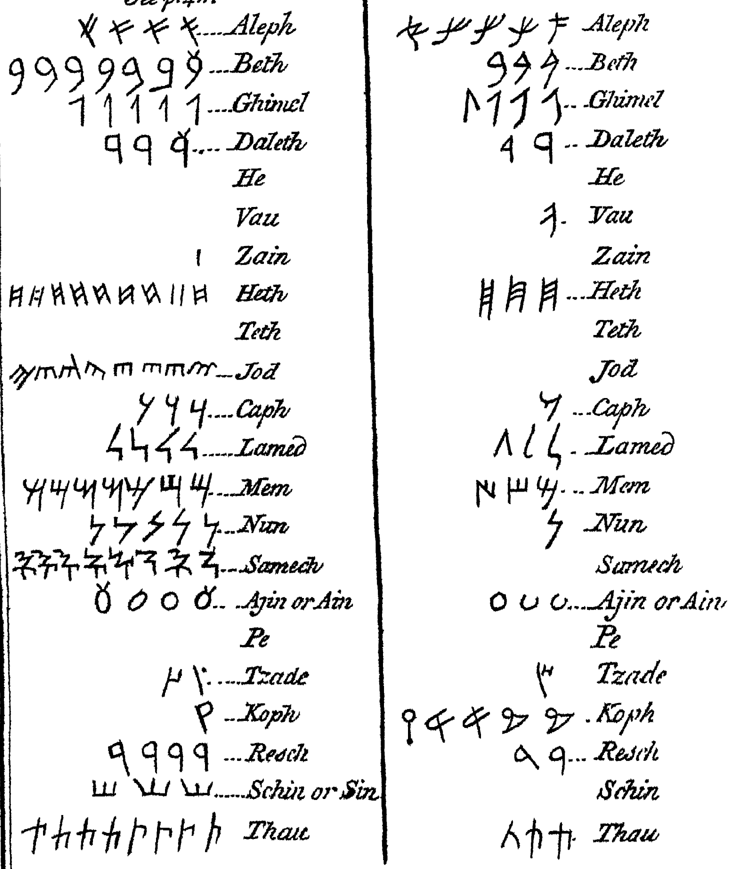
(32) Eraſm Frœl. in *Notit. Elementar. Numiſm.* paſſ. Filippo Parut. in *La Sicil. deſcrit. con Medagl. &c.* paſſ. In Roma, 1649. Vid. etiam Hubertum Goltzium, in *Sicil. Numiſm.* paſſ. Antverpiæ, 1617.

(33) Eraſm. Frœl. in *Notit. Elementar. Numiſm.* p. 83. Viennæ, Pragæ, & Tergeſti, 1758. Parut. et Goltz. ubi ſup.

the

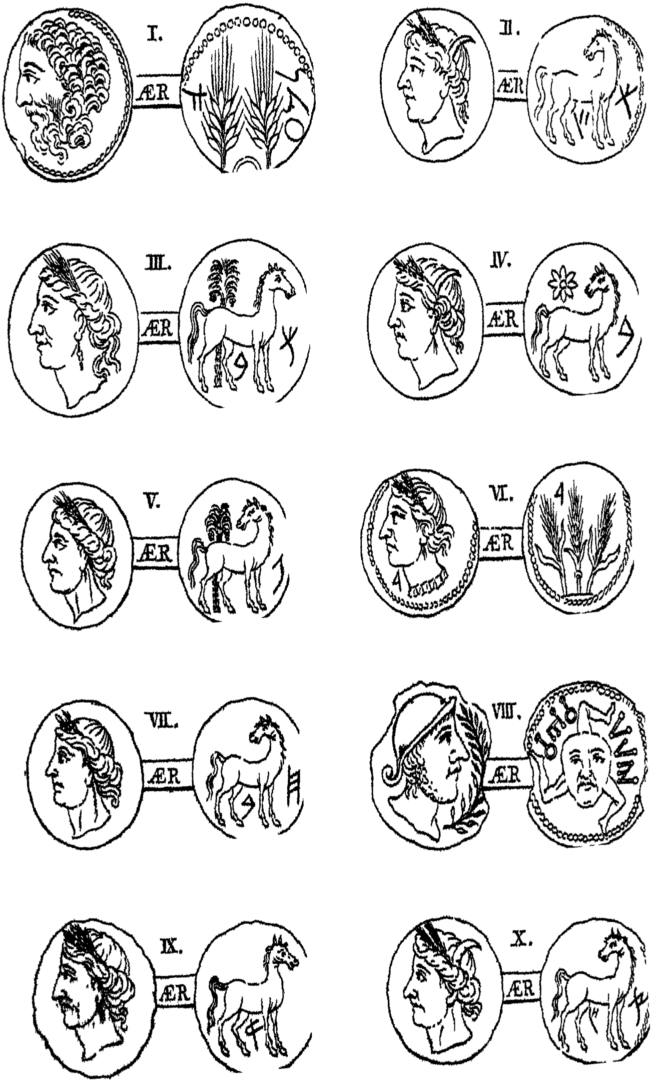
The Phœnician *Alphabet, deduced from the* Citiean *Inscriptions.* *See p. 411.*

The Siculo-Punic *Alphabet, deduced from my* Carthaginian *or* Sicilian *Coins.*



J. Mynde sc.

SICULO-PUNIC *Coins*.



the initial letter of GELA, the name of a town of Sicily, with ſeveral of whoſe (34) coins the cabinets of the curious are adorned.

Another of my Punic medals, ſtruck in Sicily, preſents to our view the two elements *Aleph* and *Beth*. Theſe plainly point at the ABACÆNA of (35) Ptolemy, an antient town of Sicily; many remains of which, according to Cluverius, were ſome time ſince to be ſeen.

One of the Siculo-Punic coins in my ſmall collection has handed down to us two alphabetic characters, the powers of which are not yet, perhaps, ſufficiently known. The ſecond of them, however, from the great reſemblance it bears to the form of that element in the Malteſe inſcription, we may venture to pronounce *Tzade*; and the firſt, according to M. l'Abbé Barthelemy, occupies the place of *Aleph*. I am nevertheleſs rather inclined to believe, that it ought to be taken for *Koph*. This ſeems to appear from the draughts of ſeveral coins, publiſhed by (36) writers of good authority, as well as from the concurring ſentiments of thoſe writers themſelves. If this notion ſhould hereafter be adopted by the learned, the medal may be ſuppoſed to have been emitted from the mint at Catana, as *Tzade* ſometimes in power anſwers to the Latin T; but if M. l'Abbé's opinion ſhould be held more conſonant to truth,

(34) Frœl. Parut. & Goltz. ubi ſup.

(35) Ptol. *Geograph.* Lib. III. c. 4. Phil. Cluver. *Sicil. Ant.* p. 386.

(36) *Veron. Illuſtrat.* Par. Terz. cap. ſett. p. 258, 259. In Verona, 1732. Ridolfin. Venut. in *Saggi di Diſſertaz. Accademich. pubblicamente lette. nella Nobil. Accadem. Etruſc. dell' antichiſſima Citta di Cortona.* p. 35. In Roma, 1735. Parut. Laſtanoſ. &c.

from

from that at Aſſorus, the ſame element being likewiſe not ſeldom equivalent to the Latin S. On another of my Siculo-Punic medals, that letter ſomewhat reſembles the *Kappa* of the Greeks. Admitting the ſecond element to be *Tzade*, as I am fully convinced it is, both ASSORVS and CATANA may, with (37) great propriety, be conſidered as Punic proper names.

For the farther illuſtration of what has been here advanced, it may not be improper to exhibit draughts of the Sicilian medals juſt deſcribed, as well as of ſeveral others, now in my poſſeſſion, adorned with Punic characters. From theſe (TAB. XXIII.), at firſt ſight, will be deducible a Siculo-Punic alphabet [TAB. XXIV.]; which, though incomplete, may, perhaps, not prove unacceptable to the learned.

Before I conclude my remarks on the Siculo-Punic coins, publiſhed by M. l'Abbé, I muſt beg leave to repeat, that only two of them, which were ſtruck at Menæ, are produced, in order to evince the reality of his form of *He*; and that he has himſelf allowed this character, on one of thoſe medals, (which conceſſion will likewiſe extend to that on the other, as has been already obſerved) to be as properly expreſſive of *Mem* as of *He*. I ſay, his form of *He*; becauſe I cannot help taking the character he denominates *He* for *Mem*, as it ſo nearly reſembles the common and uſual form of that element, and as the ſenſe it communicates even here, ſuppoſing it *Mem*, is ſo perfectly conſonant to the tenor of ſuch inſcriptions. This will be readily admitted by every

(37) Val. Schind. *Lex. Pentaglot.* p. 113, 114, 1641. Hanoviæ, 1612.

one

one not totally unacquainted with the medallic ſcience, and not an intire ſtranger to the firſt principles of oriental literature. For מעם מחנרת, EX POPVLO MENENIO, A POPVLO MENENIO, POPVLO MENENIO, (ſcil. CVSVS, vel PERCVSSVS, NVMMVS) is certainly altogether as proper as העם מחנרת, IPSE POPVLVS MENENIVS, (ſcil. CVDIT, vel PERCVSSIT, NVMMVM) if not really much more ſo. That S. C. i. e. SENATVS CONSVLTO, D. D. i. e. DECRETO DECVRIONVM, EX D. D. i. e. EX DECRETO DECVRIONVM, &c. not ſeldom occur on antient coins, is a point too well known, (38) even to ſmatterers in antiquity, to be diſputed amongſt the learned.

III.

As my Latin diſſertation upon the ſecond Citiean inſcription, of which I have already given a Latin tranſlation, inſerted in the MARMORA (39) OXONIENSIA, is almoſt ready for the preſs; I might eaſily excuſe myſelf, at preſent, from taking any notice of M. l'Abbé's pretended explication of that inſcription. But as this, in conjunction with his attempt to interpret the fourth, is undoubtedly by far the moſt exceptionable (40) part of his whole performance, ſince he ſeems totally to have miſtaken the ſenſe of both inſcriptions; I ſhall beg leave to

(38) Lud. Debiel, è Soc. Jeſ. in *Utilit. Rei Numar. Veter.* p. 99, 100, 112, &c. Viennæ Auſtriæ, 1733.

(39) *Marmor. Oxon.* Par. Secund. Tab. III. p. 7. Oxonii, 1763.

(40) *Mémoir. de Litter.* ubi ſup. p. 421, 422, 423.

submit to the consideration of the Royal Society the following short strictures upon it, reserving to myself the liberty of being more explicit and particular on this head in my future dissertation.

1. With regard to the second Citiean inscription, the third letter of the first word is not *Mem*, but *Caph*; as most evidently appears from the autograph itself, now in the possession of the University of Oxford. Farther, that אנם ought to be rendered DORMIAM, DORMIO, or JACEO, as M. l'Abbé asserts, had that been the first word of the inscription, as most certainly it is not, we shall scarce be able to learn from any lexicographer.

2. The sixth word of the first line is *Hhur*, *Chur*, or *Hur*, not *Chad*, as M. l'Abbé seems to imagine; the last letter of that word being manifestly *Resch*, and sufficiently distinguished by its length from *Daleth*, which occurs twice in the beginning of this line.

3. The term מצברת cannot bear a relation to any particular city, town, or country, as our learned antiquary is pleased to suppose; because this would imply, that the sepulchral inscription had only a single person for its object. But this notion is intirely overthrown by the word נחתי, in the second line, and מתי, in the third; both of which are evidently plurals in construction, and consequently cannot refer to less than two particular persons. Besides, as the most antient inscriptions were probably the shortest and most simple, the age of the monument itself seems to announce a duality of persons at least to have been pointed at by the inscription.

I can

4. I can at preſent ſee no reaſon why M. l'Abbé ſhould ſuppoſe this inſcription to run in the firſt perſon, rather than the third. Perhaps he will ſay that the Malteſe inſcription, as by him explained, affords us a remarkable inſtance of ſuch an uncommon mode of expreſſion. But this, I humbly conceive, is no reaſon at all; becauſe in one particular inſcription ſome peculiarities may appear, as is often the caſe, that in others do not occur. Beſides, the poſtulate he begs, or rather aſſumes, will, I am perſuaded, not be ſo readily granted him by the learned.

5. M. l'Abbé has added a fictitious *Schin* to the beginning of the ſecond line, not the fainteſt traces of which are diſcernible on the ſtone. Nay, that *Schin* could never have been there, is ſelf-evident, at firſt ſight, from the very face of the inſcription.

6. The word שלם, PAX, formed, according to M. l'Abbé, of the fictitious *Schin* and the two firſt letters of the ſecond line, was moſt certainly never a part of this inſcription.

7. He ſuppoſes an *hiatus* in the ſecond line, and another in the third; whereas not a ſingle letter is wanting in the inſcription, nor any of the words ſo effaced as to be rendered illegible, by the injuries of time.

8. For the words חרת, בם, לאמת, מתי, מכב, עלם, חיי, למכב, which clearly preſent themſelves to our view in the inſcription, he has taken the liberty to inſert מחרת, אשתי, שחת, משכב, על, בחיי, שלם, without the leaſt ſhadow of a reaſon for ſuch an arbitrary inſertion; ſeveral of his letters being purely imaginary, and not the fainteſt traces of them having ever exiſted on the ſtone.

9. The

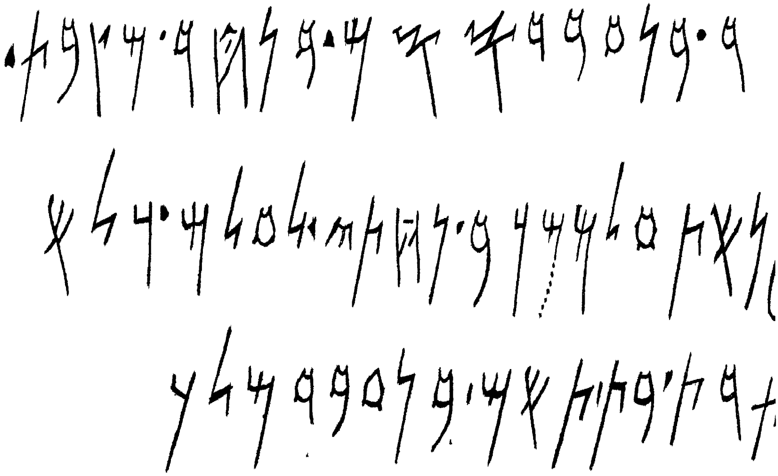
9. The name TAM, or, as he corruptly writes it, THAM, is taken by him for only part of a word; whereas it is a perfect and complete proper name, as most clearly appears from the face of the inscription, and from some antient writers of good authority, who will hereafter be referred to in these remarks.

10. He has not translated the words תררת, בם, לאמרת, מתי, כלא, לעלם, נחתי, מכב, עלם, though they form so considerable a part of this monument; and though a translation of them is so essentially necessary, in order to arrive at a true interpretation of the inscription.

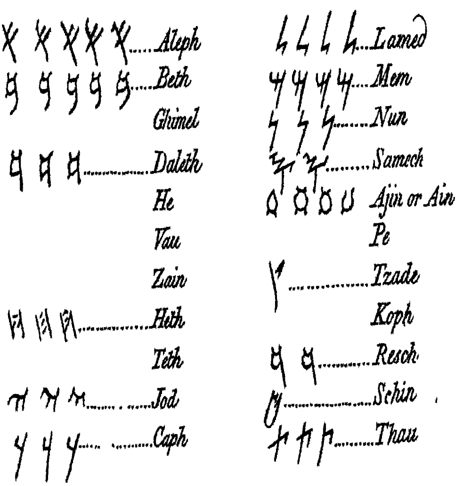
In confirmation of what has been here advanced, I shall beg leave to subjoin a short and concise explication of this inscription; which, I flatter myself, will not be found very remote from truth, as it is consonant to the faith of history, and supported by two or three Greek authors of very considerable note amongst the learned.

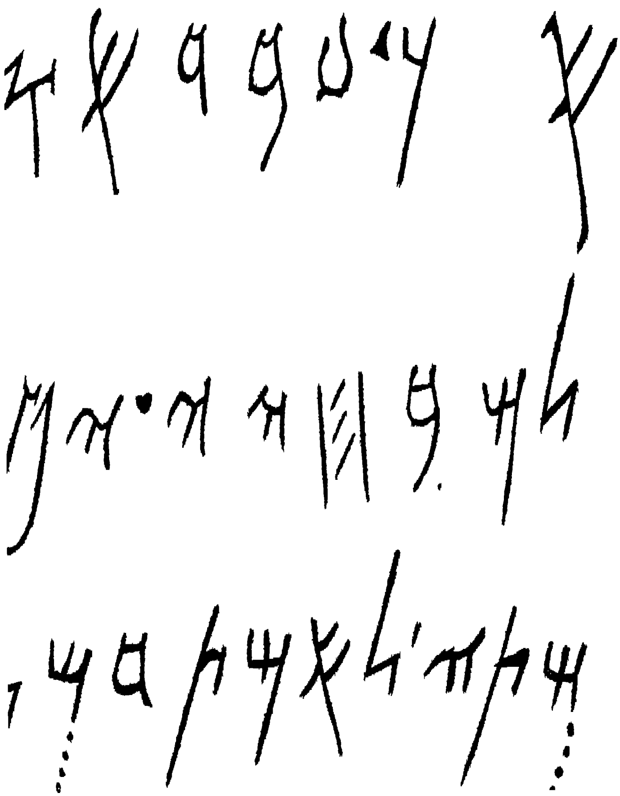
It may, however, be previously requisite to observe, that the plate hereunto annexed [TAB. XXV.] exhibits the most perfect representation of this monument, both with regard to the magnitude of the stone, and the forms and size of the letters, that can possibly be communicated to the learned world. As the copy therefore now presented to the public is the result of a fresh and most careful examination of the autograph itself, and by me, since the publication of the MARMORA OXONIENSIA, immediately deduced from it; this may be considered as the most accurate transcript of our Citiean inscription, in all respects, that has hitherto appeared.

The Oxford-Citiean *Inscription*.



The Cyprio-Phœnician *Alphabet, deduced from this Inscription.*





1. The firſt word, אנך, ANAC, or ONEC, ſeems to have denoted the ſame thing in Syriac (41) and Phœnician that ONΥΞ did in Greek, and ONYX in Latin. But the ſame ſpecies of marble was denominated both ONYX and ALABASTRITES, as we (42) learn from Pliny, and other good authors. It cannot therefore well be doubted, but that אנך, ANAC, or ONEC, here may either be tranſlated ſtrictly and literally ALABASTRITES, or be rendered with ſufficient propriety by the more general term MARMOR. The Cyprian ſtone itſelf, on which the inſcription has been preſerved, being a fine white alabaſter, or perfect ALABASTRITES, puts the point here inſiſted on almoſt beyond diſpute.

2. I have already given ſo full and particular an account of the ſecond word עבדאסר, which is a Phœnician proper name of a man, in my former remarks, (43) that it would be intirely ſuperfluous and unneceſſary to take any notice of it here.

3. That the third word, בן, BEN, which occurs afterwards in the firſt line, is equivalent to the Latin FILIVS, I have (44), in a former work, rendered inconteſtably clear.

(41) Johan. Buxtorf. Lex. Chaldaic. & Syriac. p. 25. Baſileæ, 1622.

(42) Plin. *Nat. Hiſt.* Lib. XXXVI. c. 7, 8. Lib. XXXVII. c. 5, 6. Hor. *Carm.* Lib. IV. od. 12. Martial. *Epigram.* Lib. VII. ep. 93. Dioſcorid. Lib. V. c. 153. Iſidor. Lib. XVI. c. 15.

(43) See above, p. 127, 128.

(44) *Inſcript. Cit.* p. 22. Oxon. 1750.

4. The

4. The fourth word, עבדססם, ABDESVSIM, or ABDESASIM, is another Phœnician proper name of a man, not to be met with in any antient author. If we read it ABDESVSIM, it anſwers to the Latin SERVVS, or CVLTOR, EQVORVM; if ABDESASIM, (45) to REGENTIVM (viz. DEORVM Mundum REGENTIVM) CVLTOR, or SERVVS. The latter ſeems to be by far the moſt eligible lection. Which if we admit, it will ſeem to follow, that the Phœnician term ססם, SASIM, was ſometimes at leaſt equivalent to DII, SVPERI, or rather DII Mundum REGENTES, amongſt the Latins.

5. The ſixth word, חור, HHVR, or HVR, is alſo a Phœnician, as well as a (46) Biblical Hebrew, maſculine proper name. The firſt letter, *Hheth*, here is of a pretty unuſual form. I have another Phœnician inſcription, never hitherto publiſhed, with the very ſame character in it; of which, if God grants me life and health, I intend to communicate an interpretation to the learned world.

6. The ſeventh term מצבח, from יצב, ought to be rendered in Latin LAPIS (47) SEPVLCHRALIS. It ſeems to begin a new ſentence, and to be intirely detached from the preceding words. The genuine

(45) Vid. Jac. Gol. *Lex. Arabic.* in voc. ساس, ſive ססם, REXIT PRO ARBITRIO. Sam. Bochart. *Hierozoic.* p. 16. Franequeræ, 1690. Jo. Leonhard. Reckenberg. Lex. Hebr. p. 1072. Jenæ, 1749.

(46) Matth. Hiller. *Onomaſt. Sacr.* p. 99, 789. Tubingæ, 1706.

(47) Leonhard. Reckenberg. ubi ſup. p. 690. Jenæ, 1749. Chriſtian. Stocki *Clav. Ling. Sanct. Vet. Teſt.* p. 470. Jenæ, 1727.

ſignifi-

ſignification of this word is conſonant enough to the tenor of the inſcription, which will not admit the ſenſe aſſigned it by M. l'Abbé. The figure of the *Tzade*, which is a very uncommon one, occurs in the inedited inſcription I hope hereafter to explain.

7. The eighth word, למב, LEMB, or LEMEB, is probably another Phœnician proper name. That it was uſed as a proper name in Syria, from Joſephus is (48) abundantly clear. It occurs alſo as the proper name of a man in Feſtus. The ſubſtantive בן, BEN, FILIVS, does not follow למב, LEMB, with the father's name, according to the manner of writing not infrequently ſeen in the Phœnician inſcriptions. But other inſtances of ſuch an omiſſion (49) as this have been obſerved, on ſeveral of the antient ſtones found in the ruins of Citium.

8. The verb חיי next preſents itſelf to our view, and ought to be rendered VIXIT, according to the Hebrew lexicographers. A ſuppreſſion of the relative pronoun אשר, QVI, is obſervable here. But ſuch ellipſes as this were antiently not uncommon, according to Noldius (50).

9. The following character, though in a manner the ſame with the Citiean form of *Jod*, is likewiſe ſomewhat ſimilar to the Palmyrene numeral character repreſenting TWENTY, (51) and not diſſi-

(48) Joſeph. *Antiquit. Judaic.*. Lib. XIII. c. 15. p. 599. Ed. Hudſon. Oxon. 1720. Sex. Pomp. Feſt. Lib. XVI. p. 455. Amſtelodami, 1699.

(49) *Inſcript. Cit.* 17, 21, 23, &c.

(50) Chriſtian. Nold. *Concordant. Particular. Ebræo-Chald.* p. 102. Jenæ, 1734.

(51) *Philoſoph. Tranſ.* Vol. XLVIII. Tab. XXIV. Inſcript. III.

milar to one of the equipollent characters used (52) at Sidon. As the word חיי, VIXIT, therefore manifestly requires a numeral, I have taken the liberty to translate this TWENTY. The learned world may expect a farther account of this numeral in my future dissertation.

10. The next character sufficiently resembles (53) the Palmyrene form of *Schin*, on a Parthian coin by me formerly explained, as well as in the Palmyrene alphabet. It may therefore pass for that element, though it is somewhat longer; especially, as the Syrians were neighbours to the Phœnicians, and the letter *Schin* here begins a word so consonant to the tenor of this part of the inscription. That word is apparently שנאת, which comes very appositely after חיי עשרים, VIXIT VIGINTI, or rather QVI VIXIT VIGINTI. As it appears here in construction, it is a singular; but must nevertheless be translated ANNOS, the genius of the Phœnician language in this respect being probably the same with that of the Hebrew. The *Aleph* is to be considered as a *mater lectionis*, in the term before me; the letters *Aleph*, *Vau*, *Jod*, (54) not infrequently occupying the places of vowels amongst the earlier Hebrews, and consequently, as there is great reason to believe, amongst the Phœnicians also. But of this more hereafter.

11. The Phœnician substantive עלם, as in Hebrew, is undoubtedly equivalent to SECVLVM, ÆTERNITAS, DVRATIO HOMINIBVS ABSCONDITA, &c.

(52) *Philosoph. Transf.* Vol. L. Tab. XXXII. p. 805.
(53) *Philosoph. Transf.* Vol. XLIX. Tab. XVIII. p. 593.
(54) Campeg. Vitring. *Observat. Sacr.* p. 186.

It

It frequently occurs, both in a limited and unlimited ſenſe, in the Old Teſtament; and accommodates itſelf, according to (55) Guſſetius, to the nature of the ſubject to which it is applied. Which if we admit, when applied to men, it muſt denote the term or period of human life. And in this ſignification it (56) ſometimes may be met with in holy writ, as it manifeſtly is in our inſcription. The *Vau* here, in conformity to the Phœnician cuſtom, is ſuppreſſed. But for a farther account of this word, recourſe may be had to the learned Sontagius, in (57) his diſſertation upon the terms עולם and יום, printed at Altorf, in 1695.

12. The Hebrew noun מכאוב, from whence מכאבים and מכאבות, is deduced from the (58) verb כאב, DOLVIT. By the extruſion of the two quieſcent letters, *Aleph* and *Vau*, the ſubſtantive becomes מכב, (the ſame in pronunciation with מכאוב) as it appears in our inſcription. Nor is it to be wondered at, that, before the invention of the vowel-points, the quieſcent letters ſhould have ſometimes been ſuppreſſed, as they had in reality no power at all. Inſtances of ſuch a ſyncope, or extruſion, as that here obſerved, are not ſeldom found in the ſacred writings of the Old Teſtament. And that this was really the caſe with regard to the word מכב, the Chaldee term

(55) Jacob. Guſſet. *Comment. Ling. Ebraic.* p. 1160, 1161. Lipſiæ, 1743.

(56) PSAL. LXXXIX. 1. CXIX. 44. & alib.

(57) Chriſtoph. Sontag. in *Diſſert. de* עולם *periodico &* יום *æterno*, Altorfii, 1695.

(58) Leonhard. Reckenberg. ubi ſup. p. 738.

כבא, (59) agreeing perfectly in signification with it, seems inconteſtably to prove.

From what has been advanced we may conclude, that the Phœnician words עלם מכב, on our ſtone, are equivalent to SECVLI, or VITÆ, DOLORIS, i. e. VITÆ INFELICITER ACTÆ, as I have taken the liberty to render them here.

13. The next word is נחתי, DESCENDENTES, or rather DESCENDVNT, the verb SVNT being ſuppreſſed, as amongſt the Hebrews it frequently happened to be. The radix of this participle is נחת, DESCENDIT, a verb well enough known to the Hebrews, but frequently uſed by the Chaldees; who generally applied to it the ſignification of the verb ירד, as it prevailed amongſt the Hebrews, according to (60) Buxtorf. It muſt be remarked, that נחתי לעלם כלא ought to be deemed the ſame expreſſion as לעלם נחתי כלא; the word לעלם having had different poſitions antiently aſſigned (61) it in a ſentence by the Hebrews, and therefore undoubtedly by the Phœnicians. In the Chaldee ſeveral paſſages ſimilar to this part of our inſcription are to be found. So היך נחתי גובא, SICVT DESCENDENTES IN FOVEAM ſepulchri, occurs, in PROV. i. 12. נחתי ימא, DESCENDENTES AD MARE, in ISAI. xlii. 10. And in PSAL. cxliii. 7. we meet with גוב בירת קבורתא עם נחתי, CVM DESCENDENTIBVS IN FOVEAM SEPVL-

(59) Johan. Buxtorf. F. *Lex. Chald. Talm. & Rabbin.* p. 1001. Baſileæ, 1639.

(60) Johan. Buxtorf. F. *Lex. Chald. Talm. & Rabbin.* p. 1330.

(61) F. Mar. de Calaſi. *Concordant. Sacror. Biblior. Hebraicor.* in voc. עולם, p. 592–602. Lond. 1747.

CHRI; which ſeems to be a phraſe of the very ſame import in Chaldee that the נחתי כלא, DESCENDENTES ſunt, or rather DESCENDVNT, IN CARCEREM, of our inſcription is in Phœnician. The participle נחתי, being in conſtruction here, may, however, be conſidered either as a Chaldee, Hebrew, or Phœnician word. As for the terms לעלם כלא, IN ÆTERNVM CARCEREM, they are ſo obvious and clear that they require no farther diſcuſſion in this place.

14. The two following words, מתי לאמח, with the three preceding, form the third ſentence, or part of the inſcription. The firſt of them, מתי, deduced from מוח, MORI, EMORI, and in conſtruction, is a participle converted into a noun, and may be tranſlated MORTVI, or rather OCCISI, here. In this latter ſenſe it occurs, in ISAI. xxii. 2. It may alſo be rendered HOMINES, or VIRI INSIGNES, according to Cocceius. But the former ſeems to be the moſt obvious and natural ſignification.. The ſecond of theſe words, לאמח, is undoubtedly the name of Amathus, a celebrated city of Cyprus, that was governed by it's own princes for a conſiderable period of time. The particle ל points out the genitive caſe in our inſcription, as it does in the proper names לצדן, LETZIDON, לצר, LETZOR, or LETZVR, on the Tyrian and Sidonian coins. The ſame thing may ſometimes be ſaid of this particle, when the word immediately preceding it is in conſtruction, (62) as we find manifeſtly to be the caſe here. From the inſcription before me it appears, that the Phœnician name of Amathus was אמח, AMATH,

(62) Vid. Reckenberg. ubi ſup. p. 828.

not

not חמת, HAMATH, or CHAMATH, as it has been written by Bochart (63).

15. The next word, בם, apparently to be deduced from the obsolete verb בם, ALTVM EST, ELEVATVM EST, &c. in the infinitive mood בום, from whence במה, EXCELSVM, may with sufficient propriety be interpreted MONVMENTVM SEPVLCHRALE; as במה, a word of the same origin, has actually been interpreted by (64) Schindler and Clodius. Farther, בם may be considered as no other term than the Syriac בים, SVGGESTVM, TRIBVNAL (65), TVMVLVS, &c. the excision of *Jod* being common, as in צרן, for צירן, and צרנם, for צירנים, amongst the Phœnicians. Nor is the signification assigned בם here more consonant to the true import of the radix, from whence it is derived, than to the tenor of that part of the inscription to which it belongs.

16. That the Phœnicians wrote the Hebrew word טירה, STRVCTVRA, or rather STRVCTVRA ORDINATA, תרה, and in construction תרת, cannot well be denied; since טירה was no other (66) term than תור, or תר, and it was common with the Phœnicians to expunge *Jod*, as has been just observed. The verb substantive, in conformity to the Hebrew and Phœnician custom, has been apparently suppressed here.

(63) Sam. Bochart. *Chan.* Lib. I. c. 3.

(64) Val. Schind. *Lex Pentaglot.* p. 171. Hanoviæ, 1612. Jo. Christ. Clod. *Lex. Hebraic. Select.* p. 72. Lipsiæ, 1744.

(65) Johan. Buxtorf. *Lex. Chaldaic. & Syriac.* p. 54. Basileæ, 1622.

(66) Reckenberg. ubi sup. p. 598, 1563.

17. For

17. For בירת the Carthaginians wrote ברת, without *Jod*, as we learn from the Punic inſcription explained in (67) a former paper. And that the Phœnicians wrote this word in the ſame manner, conſidering their frequent omiſſion of *Jod*, I ſee not the leaſt reaſon to doubt. The Ethiopians in this term moſt certainly never made uſe of that element. In our inſcription it is to be therefore conſidered as equivalent to DOMVS, GENS, FAMILIA, &c. in which ſignification it not ſeldom occurs; ſometimes being applied to a whole family, and at other times to a part of a family only, in the (68) ſacred writings of the Old Teſtament. The three laſt words of the inſcription, תאם בן עבדמלך, TAMI FILII ABDEMELECI, are ſo intirely clear of all difficulties, and ſo obvious to every one in the leaſt acquainted with antient hiſtory, and oriental literature, that it would be ſuperfluous to expatiate upon them here.

It may, however, not be improper to remark, that the uſe of the proper name ABD'ALMALEC, the ſame with ABDEMELEC, was retained by the Arabs long after the firſt appearance of our inſcription. For the Khalîf Abd'almâlec, who ſucceeded Merwân I. above half a century after the death of Mohammed, departed this life (69) in the year of the Hejra 86, or of CHRIST 705. Nay, 'tis more than probable, that the ſame name prevails amongſt the Arabs even at this day.

(67) *Philoſ. Tranſ.* Vol. LIII. p. 275, 276.
(68) Reckenberg, ubi ſup. p. 130.
(69) Al Makîn, Greg. Abu'l Faraj, Eutych. Iſm. Abu'lfed. Ebn Al Athîr, aliique ſcriptor. Arab.

18. If what has been here advanced ſhould meet with the approbation of the Royal Society, the following Latin and Engliſh verſions of this inſcription, which has ſo well eſcaped the injuries of time, may not prove unacceptable to the learned.

אנך עבדאסר בן עבדססם בן חר—מצבת
למב חיי כ שנאת עלם מכב—נחתי לעלם כלא
מתי לאמת—בם תרת בת תאם בן עבדמלך

MARMOR ABDASARI FILII ABDESASIMI FILII HHVRI—LAPIS SEPVLCHRALIS LEMBI (vel LEMEBI) QVI VIXIT VICENOS ANNOS SECVLI DOLORIS (i. e. ÆTATIS ſive VITÆ INFELICITER ACTÆ)—DESCENDVNT IN ÆTERNVM IN CARCEREM ſepulchri MORTVI hi AMATHVNTIS (ſeu potius OCCISI hi AMATHVSII)—MONVMENTVM STRVCTVRA eſt DOMVS (vel FAMILIÆ) TAMI FILII ABDEMELECI.

THE MARBLE (or MARBLE TOMB-STONE) OF ABDASAR THE SON OF ABDESASIM THE SON OF HHUR (or HUR)—THE SEPULCHRAL STONE OF LEMB (or LEMEB) WHO LIVED TWENTY YEARS IN TROUBLE AND SORROW — Theſe AMATHUSIANS WHO WERE SLAIN ARE GONE FOR EVER TO THE PRISON of the grave—THE MONUMENT WAS ERECTED BY THE HOUSE OF TAMUS (or TAM) THE SON OF ABDEMELEC.

Hence it ſeems to appear, that the names of two Amathuſians, probably of the firſt diſtinction, one of whom was unfortunate enough, have been handed down

down to us, and perhaps to all ſucceeding ages, by this ſepulchral inſcription.

19. It muſt be farther obſerved, that this curious monument conſiſts of four ſhort periods; every one of which may, in ſome reſpect, be taken for a complete inſcription. But this is a property it has in common with other ſimilar remains of antiquity. Thus the Sigean inſcription (70) is compoſed of four ſuch periods, and three are exhibited by the Punic (71) inſcription that in a former paper I have attempted to explain.

20. I have hinted above, that the inſcription before me is come down to us perfect and incorrupt; not ſo much as one of its letters having been either loſt, or greatly damaged, by the injuries of time. To which I ſhall now beg leave to add, that the words formed of theſe letters are, for the moſt part, diſtinguiſhed from one another by points, placed between them; which muſt, in a good meaſure at leaſt, aſcertain the lection here, and of courſe greatly facilitate the explication. The Etruſcans ſometimes ſeparated their words from one another by two points, and ſometimes by a ſingle one only, as we learn from the Etruſcan inſcriptions on the celebrated tables of Gubbio, and others publiſhed by Sig. Gori, in (72) the learned work referred to, which may be conſidered as a noble repoſitory of all kinds of Etruſcan antiquities. The earlier Greeks alſo uſed the firſt kind of interpunction, as we learn from the

(70) Chiſh. *Antiquitat. Aſiat.* p. 30, 31. Lond. 1728.
(71) *Philoſ. Tranſ.* Vol. LIII. p. 279.
(72) Anton. Franciſc. Gor. *Muſ. Etruſc.* Vol. I. II. paſſ. Florentiæ, 1737. & Vol. III. paſſ. Florentiæ, 1743.

 Sigean,

Sigean, (73) Teian, and other antient inſcriptions. That they likewiſe applied three points for the ſeparation of their words, on (74) ſome occaſions, tho' more rarely, as well as the Etruſcans, is not unknown to thoſe who have been converſant with the antiquities of theſe nations. I muſt farther obſerve, that this minute kind of mark, though generally termed a point, was originally of a triangular form; as may be inferred both from our Citiean inſcription, in which ſome of the minute black triangles plainly appear, and one at leaſt of thoſe preſerved by the tables of Gubbio (75), of which ſo accurate a tranſcript has been communicated by Sig. Gori to the learned world. That theſe points are a certain indication of a pretty remote antiquity, is by the moſt competent judges of ſuch matters (76) readily allowed. How far therefore this interpunction and antient hiſtory may conſpire, in order to ſettle the age of the monument under conſideration here, I am next to inquire.

Abdemon, the Citiean, one of the Perſian (77) monarch's friends, having been expelled Salamine by Euagoras, that prince meditated the reduction of

(73) Chiſh. *Antiquitat. Aſiat.* p. 6, 14, 97, 98. Paul. M. Paciaud. *Monument. Peloponneſ.* 207, 209—213, 218. Romæ, 1761.

(74) *Muſ. Veronenſ.* p. 407. Veronæ, 1749. Anton. Franciſc. Gor. *Muſ. Etruſc.* Vol. III. P. III. T. XVI. Florentiæ, 1743.

(75) Anton. Franciſc. Gor. *Muſ. Etruſc.* Vol. I. Prolegom. p. 55. Florentiæ, 1737.

(76) Vid. Chiſh. *Antiquitat. Aſiat.* p. 3, 6, 14, 97.

(77) Theopompus in Excerptis Photii, Cod. CLXXVI. *Inſcript. Cit.* p. 24—28. Oxon. 1750. Diod. Sic. *Bibl. Hiſt.* Lib. XVI. p. 447. Iſocrat. *Evag.* p. 282.

the

the whole iſland of Cyprus; in which, within the courſe of a few years, he made a very conſiderable progreſs. This alarming the Amathuſians, Citieans, and Solians, governed then, as it ſhould ſeem, by their own princes, they (78) made the proper diſpoſitions for oppoſing his ambitious deſigns. But not believing themſelves able alone to cope with him, they applied to the Perſian (79) court for aſſiſtance. Artaxerxes Mnemon, who then ſat upon the Perſian throne, was alſo himſelf become jealous of the growing power of Euagoras, and therefore readily entered into an alliance with the three confederated cities againſt him. To this he was farther excited by the murder of Agyris, king of Amathus, and one of his moſt faithful allies, of which Euagoras (80) was accuſed; and by the engagement the three Cyprian ſtates had entered into, to put the whole iſland, if poſſible, into his hands. In order therefore to cruſh Euagoras at once, Artaxerxes ſent an army of 300,000 men, under the command of Orontes, one of his ſons-in-law, to invade Cyprus (81), in the third year of the ninety-eighth Olympiad, or the year before Chriſt 386. This formidable army was attended by a fleet of above 300 (82) ſail, of which Gaus, the

(78) Diod. Sic. ubi ſup. p. 447.
(79) Id. ibid.
(80) Id. ibid.
(81) Diod. Sic. ubi ſup. Lib. XV. p. 458.
(82) Id. ibid. The Phœnician name TAM ſeems to have been written by the Greeks ΤΑΜΟΣ, as it is exhibited by a MS. of Thucydides, in the French king's library; and not ΤΑΜΩΣ, as we find it written in other manuſcripts of that author. This is rendered not a little probable, at leaſt in my opinion, by the Oxford-Citiean inſcription.

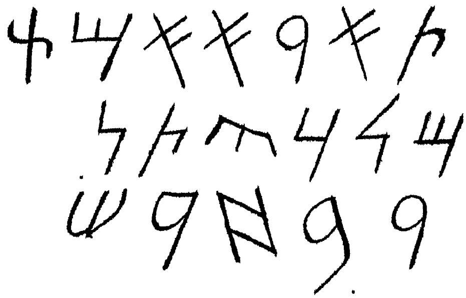
Vid. Thucydid. *De Bell. Peloponneſ.* Lib. VIII. c. 87. p. 557. Edit. Duker. Amſtelædami, 1731.

ſon of Tamus, or, as the Phœnicians wrote and pronounced the word, Tam, probably the TAM of our inſcription, was (83) admiral. This Tamus is ſaid to have been born at Memphis, and conſequenetly by birth to have been an Egyptian, though he was probably of Phœnician extraction. Being a perſon of great valour, and uncommon ſkill in maritime affairs, he firſt ſerved Tiſſaphernes as a naval officer; but was afterwards imployed by Cyrus, who rebelled againſt his brother Artaxerxes, and was killed in the battle (84) of Cunaxa, as chief commander of his fleet. He alſo had been appointed governor of Ionia by that prince. Tamus was treacherouſly cut off, with all his family, except his ſon Gaus, now the Perſian admiral, who ſtaid behind in Aſia, by Pſammitichus, king of Egypt (85), about fourteen years before. Euagoras's fleet of 200 ſail was defeated near Citium (86) by Gaus, the ſon of Tamus, or Tam, with the loſs of moſt of his ſhips; though Euagoras had, before this naval engagement, gained a (87) conſiderable advantage over a part of the combined army of Perſians, Amathuſians, Citieans, and Solians, almoſt immediately after the deſcent had been made. From this ſhort narrative, extracted from writers of the beſt reputation and authority, are naturally deducible the following obſervations.

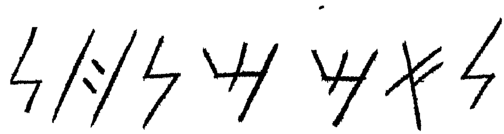
(83) Id. ibid.
(84) Xenoph. *De Cyr. Expedit.* p. 89. Oxon. 1735. Plutarch. in *Artaxerx.* p. 1014, 1015. Lutetiæ Pariſiorum, 1624.
(85) Diod. Sic. *Bibl. Hiſt.* Lib. XIV. p. 415.
(86) Diod. Sic. *Bibl. Hiſt.* Lib. XV. p. 459, 460.
(87) Idem ibid.

Three of the Citiean *Inscriptions illustrated here. p. 429, 432, &c.*

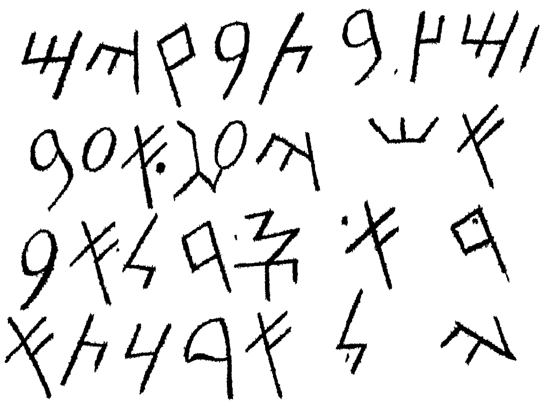
N°. 3.



N°. 21.



N°. 23.



1. Tamus, or Tam, probably the TAM of our inscription, admiral of a Persian fleet, and governor of Ionia, was cut off by Psammitichus, king of Egypt, together with his whole family, except his son Gaus, about fourteen years before the commencement of the Cyprian war.

2. Gaus, the son of Tamus, or Tam, admiral to Cyrus, who was killed on the plains of Cunaxa, actually commanded the Persian fleet, and defeated that of Euagoras, near Citium, in the beginning of that war.

3. Part of the combined army of Persians, Amathusians, Citieans, and Solians, was routed by Euagoras, a little before the naval engagement.

4. From what has been (88) intimated by Diodorus Siculus we may infer, that this action certainly happened at no great distance from Citium; as the battle by sea was fought near that place, and as the fleet and army must have acted in concert, both at the debarkation of the troops, and for some time after that event.

5. It must therefore be allowed probable, that the two Amathusians mentioned in our inscription, who seem to have been persons of distinction, were killed either in the aforesaid action, in the naval engagement that immediately followed, or in some other affair that happened much about the same time.

6. The monument recorded by our inscription was probably erected by some of Gaus's family, who might call themselves the house of Tamus, his father, several instances of such an appellation occurring

(88) Diod. Sic. ubi sup. p. 459, 460.

in antient hiſtory. This might have happened after Gaus's death, which was about two years poſterior to the commencement of the Cyprian expedition. The erection of it certainly ought not to be attributed to Tamus's daughter, as ſome may perhaps pretend; all that admiral's family, except Gaus, having been cut off with him, (89) by Pſammitichus, king of Egypt, fourteen years before.

7. From the preceding narrative we may infer, that antient hiſtory, particularly that of Diodorus Siculus, from whence it is chiefly extracted, and our inſcription mutually ſtrengthen and ſupport each other.

8. Hence it ſeems pretty clearly to appear, that the death of Abdaſar and Lemb, or Lemeb, the event commemorated by our inſcription, preceded the commencement of the Chriſtian æra 386 years; and conſequently that this inſcription is coeval with thoſe, found likewiſe in the ruins of Citium, by me ſome years ſince explained.

Many things relative to this inſcription, for want of room, I am obliged at preſent to ſuperſede; but theſe, as well as ſeveral other points ſlightly touched upon here, may perhaps meet with a more particular diſcuſſion, in another piece upon the ſame ſubject; which, if God grants me life and health, will ſoon be communicated to the learned world.

As the autograph of the fourth inſcription [Tab. XXVI.] does not now exiſt, having been deſtroyed by Bekir, baſhaw of Cyprus, about the year 1749; we cannot arrive at any degree of certainty, in relation to the

(89) Diod. Sic. *Bibl. Hiſt.* Lib. XIV. p. 415.

characters of which it was originally compoſed. Of thoſe, however, exhibited by the tranſcripts only the laſt of the firſt line and the laſt of the inſcription ſeem to have deviated from their primitive forms. The laſt character of the firſt line I can by no means take for *He*, as M. l'Abbé Barthelemy ſuppoſes it to be, ſince it ſcarce bears a remote reſemblance even to his pretended new form of that element. Beſides, M. l'Abbé has in effect given up this new form, by allowing that on the coins of Menæ it may be taken for *Mem*. Nor can ſuch an uncouth proper name as תאראאמה, THARAAME, with three *Alephs* in it, and two together in the middle of it, I believe, be found in the whole circle of Hebrew, Syriac, or Phœnician antiquity. I ſhould therefore rather call it *Thau*, one form of which it greatly reſembles, if part of the curve behind be conſidered as an accidental addition; which might eaſily have been made, in the courſe of ſo many ages. With regard to the laſt letter of the inſcription, this ſeems to be purely adventitious, and of a recent date. For Dr. Porter's accurate copy of this inſcription, taken upon the ſpot, which the late Rev. George Drake, M. A. and Fellow of Balliol College, received of Charles Gray, Eſq; Member of Parliament for Colcheſter, and gave to the Univerſity of Oxford, exhibits a character reſembling the modern form of *Schin*, though made in a very bungling irregular manner. In ſhort it preſents to our view not the leaſt appearance of antiquity. Nay, it ſeems to have been formed upon the ruins of a *Nun*, the upper part of which is plainly viſible in this character. I would therefore, with the permiſſion of the critics, read the

laſt

tional light upon part of the preceding remarks, and more clearly evince a point of considerable importance, with regard to the true explication of the Oxford inscription, that has been manifestly opposed by M. l'Abbé.

1. The twenty-first of the Citiean inscriptions, which consists only of the two words לאממן חל, or לאממן חלל, had probably lost a *Lamed*, before the autograph itself was destroyed; unless we will suppose the Phœnicians of Cyprus, when it first appeared, to have used even the participle pahul itself of the radix חל, or חלל, in the contracted form. The original Phœnician is equivalent to the Latin AMAMONO INTERFECTO, AMAMONO in acie INTERFECTO, or AMAMONI in prælio CONFOSSI, scil. lapis sepulchralis, *i. e.* the grave-stone OF AMAMON KILLED in war; which seems to imply, that this Amamon, who was probably an officer of some note, fell in the affair that happened between a part of the combined army of Persians and Phœnicians and a body of Euagoras's troops (92) near Citium, soon after the commencement of the Cyprian war. Whence we may infer, that this inscription is coeval with those I have already endeavoured to explain; that it points, clearly enough, at the same event; and consequently that it brings a fresh accession of strength both to my opinion of the age of those monuments, and also to the authority of Diodorus Siculus himself in the point before us.

As for the Phœnician proper name AMAMON, I have formerly expatiated so (93) largely upon it,

(92) Diod. Sic. *Bibl. Hist.* Lib. XV. p. 459, 460.
(93) *Inscript. Cit.* p. 20, 21, 22. Oxon. 1750.

that

that nothing farther relative to this word will be expected from me here. The figure of the *Hbeth*, according to Dr. Porter's tranſcript, not a little reſembles the unuſual form of the ſame element as preſented three times by the Oxford-Citiean marble to our view.

2. The twenty-third Citiean inſcription is formed of ſix words, five of which are evidently proper names. They are ranged in four lines, and may be repreſented by Hebrew characters thus.

מצברת רקים
אסיעוא עב
דאסר לאר
ילא רכתא

LAPIS SEPVLCHRALIS RECIMI
ASIAVÆ AB
DASARI LAR
ILÆ RECATÆ

The fifth letter of the ſecond line, which ſeems to have been intended for *Vau*, is pretty much deformed. This appears from Dr. Porter's accurate copy of the inſcription. The original itſelf does not now exiſt. I take this character to have repreſented (94) *Vau*, becauſe it ſomewhat reſembles the Æolic *Digamma*, which anſwered to that element. The minute ſtrait line preceding the firſt word is evidently an accidental blemiſh, and therefore cannot be conſidered as a letter. This, from its ſize

(94) Chiſh. *Antiquitat. Aſiat.* p. 17, 19. Lond. 1728.

and poſition, as well as from the term that immediately follows it, is inconteſtably clear. The two words forming the firſt line of the inſcription are apparently מצבת רקים, LAPIS SEPULCHRALIS RECIMI, THE TOMB-STONE OF REKIM, or REKEM, the latter of which is (95) a Biblical proper name. The third of the proper names preſerved by this inſcription, ABDASAR, occurs on the Oxford and Malteſe ſtones, and has been already explained; but the others I remember not elſewhere to have ſeen, nor are they, as I apprehend, to be met with in any antient author. The perſons that bore them were probably killed in the action near Citium, mentioned by Diodorus Siculus, referred to on a ſimilar occaſion in theſe remarks, and buried in one grave. That action, as I have formerly obſerved, preceded the commencement of the Chriſtian æra about 386 years.

The ſubſtantive מצבת, in the beginning of a ſentence, immediately preceding the proper name of a man, not followed by the term בן, BEN, SON, and the father's name, appears in this ſepulchral inſcription, that has more than one perſon for its object. This word alſo occurs, attended by the very ſame circumſtances, in the firſt line of our Oxford inſcription. Now in the Citiean monument before me it is undoubtedly equivalent to the Latin LAPIS SEPULCHRALIS, and the Engliſh TOMB-STONE. Why then ſhould it not have the ſame ſignification aſſigned it in the other? Moſt certainly it ſhould. Can any thing therefore be more forced and unnatural than M. l'Abbé Barthelemy's notion of this term in the

(95) NUM. XXXI. 8.

Oxford inſcription, when he makes it to refer to I know not what obſcure town called Tſabeth, the fainteſt traces of which are not to be met with in any antient writer? A notion this void of the leaſt appearance of authority, and deſtitute of the very ſhadow of a reaſon for its ſupport!

The twenty-third Citiean inſcription is alſo remarkable for the exhibition of a very antient form of *Koph*, and of a pretty unuſual one of *Tzade*, reſembling the character that repreſented the ſame element amongſt the Palmyrenes. This likewiſe ſometimes appears upon the Phœnician (96) coins. The figure of *Vau* here ſeems by ſome accident to have been deformed, before the autograph was deſtroyed. It nevertheleſs bears a ſort of rude reſemblance to the Æolic *Digamma*, which (97) owed its origin to this letter. As it has deviated, however, conſiderably from the primitive character, cut at firſt in the ſtone; I have not aſſigned it a place in the Phœnician alphabet, deduced from the inſcriptions found amongſt the ruins of Citium, and now attending theſe remarks.

Thus have I finiſhed my remarks upon M. l'Abbé Barthelemy's reflections on certain Phœnician monuments, and the alphabets reſulting from them; and endeavoured to rectify ſome miſtakes, that occur in this celebrated performance. How far I have ſucceeded in my deſign the learned world, with candour and impartiality, will decide. I have alſo attempted to explain four of the Citiean inſcriptions, in the courſe of theſe remarks; and hope the explications

(96) Joan. Baptiſt. Biancoſi, *De Antiq. Hebraeor. & Graecor. Lit.* p. 32. Bononiæ, 1748.
(97) Chiſh. ubi ſup.

given,

given, even upon the moſt critical examination, will not be found very remote from truth. The alphabets deduced from theſe inſcriptions and the Siculo-Punic coins, illuſtrated here, will, I flatter myſelf, not a little facilitate the interpretation of other ſimilar remains of antiquity. Nor will M. l'Abbé, for whoſe ſuperior merit I have a real eſteem, find the leaſt reaſon to complain of any illiberal treatment in this memoir. For as on the one hand, ever averſe to flattery, I have delivered my ſentiments with a becoming freedom, when I thought myſelf obliged to differ from him; ſo on the other, notwithſtanding the provocations received from certain authors, I have ſtudiouſly endeavoured to avoid every thing that might ſeem to have the leaſt tendency to a diminution of his character, as well as all undue warmth and aſperity of expreſſion. Nor am I conſcious of having miſrepreſented him in any one particular. Truth ſtands in no need of ſuch ſupports, nay it utterly diſclaims them; and truth is conſidered as the ſole object in view here by,

S I R,

Your much obliged,

And moſt obedient,

Humble ſervant,

John Swinton.

Chriſt-Church, Oxon.
Nov. 24, 1764.

I N D E X.

AN INDEX TO THE Fifty-Fourth VOLUME OF THE *Philosophical Transactions*.

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ERRATA.

P. 33. l. 2. *read thus:* $T = \overline{\frac{x}{x+1}}\Big|^{-1}$

14. *read* reckoned.

18. *for* $\frac{T^3}{\times 3}$ *read* $\frac{T^3}{\times^3}$

36. 11. *from the bottom*, to 8 *put* to 5.

99. ult. *for* greatly deformed, *read* somewhat deformed.

ERRATA in VOL. LIII.

P. 240. lin. 15. *from bottom*, *for* M. De la Caille's, *read* M. De la Caille's Ephemerides.

241. l. 394. *from bottom*, *for* ecliptic orbs, *read* elliptic orbits.

245. l. 9. *from bottom*, *for* lines of conjunction, *read* times of conjunction.

177. *for* מחנגות, *read* מחנות.

292. l. 8. *for* any relation, *read* any peculiar relation.